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Current Anthropology

A WORLD JOURNAL OF THE SCIENCES OF MAN

NEOLITHIC DIFFUSION RATES <i>by Munro S. Edmonson</i>	71
SEVERAL PROBLEMS IN THE STUDY OF KINSHIP SYSTEMS <i>by D. A. Olderogge</i>	103
ARE AFRICAN CULTIVATORS TO BE CALLED "PEASANTS"? <i>by L. A. Fallers</i>	108
THE OBSIDIAN DATING METHOD <i>by Donovan L. Clark</i>	111
MORE ON MIDDLE PLEISTOCENE HOMINIDS	117
TWO NEW PROJECTS:	
<i>The Current Anthropology</i> DICTIONARIES	121
ASSOCIATES IN CURRENT ANTHROPOLOGY	145
CALENDAR	inside back cover
CONFERENCES	139
CONGRESSES	139
CORRESPONDENCE	136
FOR SALE	138
INSTITUTIONS	136
OUR READERS WRITE	69
PERSONNEL WANTED	136
RECOMMENDED TO READ	138
RESEARCH REPORTS	114
SERIAL PUBLICATIONS	138
VIKING FUND MEDALISTS OF 1960	136
WANTED	136

Current Anthropology

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Журнал "Каррент Антрополоджи" выходит шесть раз в год на английском языке и предназначен для ученых всего мира, занимающихся антропологическими науками, включая собственно антропологию, этнографию, изучение материальной и духовной культуры народов, лингвистику, археологию и исследование доисторической эпохи. Ассоциативные члены журнала "Каррент Антрополоджи" образуют группу ученых, которые сотрудничают в журнале и обмениваются через него знаниями и идеями, посвящая себя делу осуществления полного и свободного обмена знаниями в мировом масштабе. Учреждения, которые принимают приглашения стать групповыми ассоциативными членами журнала "Каррент Антрополоджи", берут на себя такие же обязательства. Обе категории ассоциативных членов журнала получают журнал и платят за него только номинальную стоимость (20% подписной цены), поскольку их активное участие в журнале предполагает дополнительные расходы. Ассоциативные члены журнала могут рекомендовать студентов, изучающих основные антропологические дисциплины, и ученых, работающих в связанных с антропологией отраслях науки, в качестве подписчиков журнала по специальной подписной цене (40% обычной подписной цены). Остальные читатели подписываются на журнал по обычной подписной цене (см. 4-ю стр. обложки).

ИСТОРИЯ ЖУРНАЛА.

Начиная со дня своего основания в 1941 году, Фонд Веннер-Грена для антропологических исследований признал ценность научного обмена в мировом масштабе между отраслями наук, предметом изучения которых является человек. Так, в 1952 году это учреждение выступило инициатором проведения международного симпозиума по антропологии, результатом которого явилась публикация "Антрополоджи Тудей" (Антропология сегодня), "Ашпрайз оф Антрополоджи Тудей" (Оценка современного состояния антропологии) и "Интернэшнл Дайректори оф Антрополоджикал Инститюшен" (Международный справочник учреждений, занимающихся изучением антропологии). За этими изданиями в 1955 году последовала публикация экспериментального "Ежегодника антропологии", часть которого была вновь переиздана в 1956 году под названием "Каррент Антрополоджи" (Новости антропологии), откуда и идет название настоящего журнала.

Преследуя ту же цель, в 1957 г. фонд основал журнал "Каррент Антрополоджи". На конференциях и в переписке с учеными всего мира обсуждалась возможная форма журнала. Конференция, которая учредила Бург Вартенштайн (в Австрии) центром Фонда Веннер-Грена в Европе, выработала следующие общие принципы деятельности журнала "Каррент Антрополоджи".

1. Журнал должен охватывать все проблемы антропологических наук, публиковать различные мнения и данные по всем вопросам, способствуя международному научному обмену.

2. Журнал должен быть целенаправленным.

3. Журнал должен являться для антропологов средством обмена информацией по вопросам их текущей работы.

В течение следующего года состоялись неоднократные встречи с антропологами всего мира и с их помощью был разработан план, в основу которого легли вышеперечисленные принципы.

ПОЛОЖЕНИЕ О ЧЛЕНСТВЕ.

Журнал "Каррент Антрополоджи" объединяет ученых и научные учреждения независимо от их национальной или политической принадлежности.

В связи с тем, что ассоциативные члены получают журнал по номинальной цене, предполагается, что они, в свою очередь, будут принимать участие в работе журнала, рекомендуя новых ассоциативных членов, представляя статьи, информацию и другие материалы, а также присылая по просьбе редакторов отзывы на статьи.

СОДЕРЖАНИЕ ЖУРНАЛА.

Журнал "Каррент Антрополоджи" публикует две категории материалов: обзорные статьи, а также информационные и справочные материалы. Несмотря на то, что журнал "Каррент Антрополоджи" несомненно будет печатать некоторые статьи, которые могут быть опубликованы в других журналах, широкое дублирование других журналов будет, однако, маловероятным, так как сфера интересов журнала в географическом и теоретическом пластах очень широка. Назначение журнала "Каррент Антрополоджи" как своеобразного центра (трибуны) для всех ученых, занимающихся антропологическими науками, является новым.

ОБЗОРНЫЕ СТАТЬИ.

К этой категории относится широкий обзор суммы знаний по любой теме, имеющей отношение к наукам, предметом изучения которых является человек. Новые материалы по развивающимся отраслям антропологии и новые оценки предпочитают синтезу широко известных сведений, однако новое должно быть увязано с уже известными фактами. Обзорной статьей может считаться обзор литературы, данных исследовательской работы, метода исследований и т.д. Рамки такой статьи должны определяться эпохой, географическими данными или любым другим критерием. Статья, как правило, должна содержать описание исходных позиций или краткое изложение того, что сделано в прошлом по данному вопросу. Это и будет служить базой для дальнейшего изложения.

Тема обзорной статьи должна быть широкой по кругозору. Раскрыть тему следует так, чтобы она была понятна, хорошо иллюстрирована конкретными фактами, ясна по изложению и нацелена в будущее. Статья должна содержать обширную библиографию. Автор, поскольку он является специалистом в одной области и обращается к специалистам в других областях, должен стремиться к простоте и ясности в изложении материала и, в частности, в терминологии, используемой для изложения его взглядов. Величина статьи определяется материалом, который она охватывает, при условии, что в отборе материала соблюдаются разумные каноны. Иллюстрации к статьям весьма желательны.

Обзорные статьи для "Каррент Антрополоджи" проходят "К.А. редакционную обработку". Это означает, что после того как статья временно принимается в журнал, с нее снимают копии и рассылают на отзыв ряду ассоциативных членов, которые являются либо знатоками в этой области, либо проявляют к ней явный интерес. Подобный метод является первым шагом в установлении конструктивного общения, которое является целью журнала. Несмотря на то, что отзывы выполняются по просьбе редакции, они рассматриваются как "рукописи" и вопрос об их публикации соответственно решается по усмотрению редактора.

Авторы отзывов подбираются по рекомендации автора статьи, а также на основании редакционной картотеки, где имеются карточки, заполненные каждым ассоциативным членом и отражающие его индивидуальные интересы. Такому автору предлагают дать два отзыва: первый должен содержать исправления или добавления, которыми автор статьи может воспользоваться по своему усмотрению; во втором — предметом обсуждения должны явиться подход к проблемам и

Our Readers Write

I feel a strong kinship with the anonymous Englishman who deplored the draining away of Wenner-Gren funds to support a periodical. Aside from the question of a professional's voice or "rights" vis-à-vis those of a contributing foundation, it is my belief that money for research and the training of professionals is more sorely needed now than it may be at some time in the future.

Would it not be wise, after 18 months or 2 years of operation, for CA to present a rough cost accounting to all Associates? The individual Associate could thus match the lost research potential from Wenner-Gren funds against what he has received through his participation in CA. Perhaps a pro and con discussion in CA would be in order. The question is worth much thought from all of us who are concerned with the needs of anthropology, and with what we think it should do and become.

DOUGLAS OSBORNE
Mesa Verde National Park, Colo.,
U.S.A.

Unless the letter from the "young English Associate" was inadequately quoted in Letter to Associates No. 6, it seems to express a frenzied devotion to "research on disappearing cultures" to the exclusion of all other anthropological activity. It ignores the sensible dictum presented in the early 1930's by Alexander Lesser in "Problems vs. Subject Matter as Directives of Research"; and it suggests an overconcern with descriptive accounts as if these were opposed to or unrelated to our capacities, as scientists of human behavior, for logical generalization and theorizing. The library shelves are lined with descriptive accounts: why not do something with what we have?

This is not to argue against empirical data-gathering, but only to question doing this for the sake of gathering data. Whether the cultural systems dealt with are disappearing, newly emerging, or merely changing, empirical descriptions from the field are justifiable only to the extent that they advance knowledge of cultural forms and processes and of human behavior. They are by no means

mutually exclusive vis-à-vis theoretical syntheses.

Nor should all Foundation funds be channelled in a single direction. CA is one of the most significant modern developments in anthropology precisely because it makes generalization possible among scholars from different traditions, working on different problems and in different regions. Its brief achievements bode well for the realization of its splendid potentialities.

JAMES SILVERBERG
Milwaukee, Wis., U.S.A.

I wonder if it is necessary for the review articles to include quite such lengthy comments? Earlier, you asked me to comment on J. Desmond Clark's paper, which I did with much pleasure. But it occurs to me that cases can easily arise where commentators find themselves completely at odds with the author. In such cases, the possibility of publication may deter them from expressing a forthright opinion.

Your system of refereeing learned papers differs from our own, in that your authors see the referee's comments and know his name. In this country, refereeing is anonymous. Personally, I prefer your system (and I have had experience with both), but I wonder if CA is not going a little too far in starting what amounts to a public debate. Perhaps this is a good thing—I merely put forward another point of view.

ROGER SUMMERS
Bulawayo, Southern Rhodesia

I should like to say that I have been impressed by the recent standard of contribution to CURRENT ANTHROPOLOGY, and expect the news page to become of increasing value. I feel, however, that it would be honest to note also that I have heard the following, in my view fairly severe, criticisms of the journal:

1. That there is too much "gimmickry";
2. That the editorial manner is intrusive and sometimes boring ("fan-magazine style" is mentioned);
3. That smartness of presentation is mistaken for more basic virtues. An ex-

ample given is the phrase "the CA² treatment" itself, referring to a fairly pedestrian, although sometimes useful, process. Another example is the use of "Bharat," "Deutschland," etc., in the price-list, in which the accuracy is said to be spurious and to create confusion where none existed:

4. That published letters contain too much unconsidered jargon in proportion to ideas. This criticism I think, had most relevance to the earlier issues;

5. That the style, outside most of the actual contributions, is slipshod, and even at times unintelligible. I personally think this criticism has some truth, and that it lies at the bottom of the other criticisms. The English language as one sees it often used in CURRENT ANTHROPOLOGY (why continually call it CA?) has lost precision and has become a floppy, useless tool. The confusion between "area of interest" and "field of specialization" was only one example of what many rigorous scholars would consider an unforgiveable woolliness. My own complaint is that for a journal which aims for world prestige it falls in this respect far below world standards. It has been genuinely embarrassing to have people of an all-round European education, as well as with interests in anthropology, picking up CURRENT ANTHROPOLOGY and reading pieces out as humour. This reaction is only too common. There is an impression that the journal is unconsciously partly directed to the American school-leaver, or the Chamber of Commerce, or to some other irrelevant group. It certainly underestimates the critical standards of a world audience;

6. That the polling of opinion is either a waste of time, since majorities are irrelevant because of inequalities in ability or training, or, when it comes to motto designs, just frivolous.

These are the criticisms of CURRENT ANTHROPOLOGY which I have heard or support myself. I do not think they are uncommon, although I have not yet seen all of them mentioned in the journal. One should be distrustful of anything that savours of the "science is fun" branch of the "education is fun" movement. This has already lowered standards to such an extent that one now tends actually to be afraid of giving offence by voicing this particular type of criticism. [This letter was not originally intended for publication, but is published with permission.—ED.]

EDWIN ARDENER
Buea, Southern Cameroons

Please continue to feature articles that summarize broad topics. These are invaluable to the specialist who wishes to keep up with developments in related fields, but is prevented from doing so

by his commitments to field work. While specialized articles are certainly of great importance, they have many other outlets for publication in journals and reviews dealing with narrower fields.

AUGUSTIN DELGADO
Mexico, D.F., Mexico

The ethnographers of Cracow, who have given me authority to write in their behalf, feel that at this time it is inevitable—but not unwelcome—that most articles published in *CA* should be written by Americans. We hope to present our work at a later date. The linguistic obstacles are very real, since European ethnographers are on the whole little acquainted with the literature and theoretical approaches of the English-speaking countries.

However, we should like to add that in our opinion it might be a good policy if, in addition to the present type of survey article, a somewhat lighter type of discussion essay could be included in *CA*. Introducing new and perhaps even controversial ideas, for which the Editor need not be held responsible—except of course for seeing to their high scientific level—such essays would in our opinion materially enhance the value of *CA* and make it more readable. Cultural anthropology versus traditional European ethnography; the concept of folk culture as applied to Hispanic America and to the old European peasant society; and the meaning of folk-art are a few examples of such topics. There are, of course, many others.

ANDRZEJ WALIGÓRSKI
Cracow, Poland

When one considers that the great majority of anthropologists in the United States and Canada, and perhaps in other countries as well, are engaged in teaching as members of academic faculties, it would seem appropriate for *CA* to recognize this fact in some way. It is perfectly true that everything published in *CA* at present assists the teacher in some manner or other. However, in planning some of the articles, the specific needs of the teaching anthropologist might be taken into account to an extent greater than at present.

For example, some of my colleagues have recently complained that there is no general article covering social strati-

fication among tribal peoples. A number of excellent empirical studies of particular cases exist, but no one has yet summarized these and offered a theoretical interpretation of stratification among tribal peoples compared with stratification in urban society. No course in social anthropology is complete without such an article, and teachers of social anthropology would heartily welcome one.

From time to time, *CA* might also publish outlines and reading lists for important courses. I have no doubt that course subject matter is undergoing an impressive revision in many universities; and I, for one, would be most happy to know more about this movement than can be gleaned from a study of college catalogs. At the moment, I am working on a similar problem for David Mandelbaum's project on "Educational Resources in Anthropology," and it occurs to me that *CA* might undertake to carry on the objectives of this project, at least as a sideline.

JOHN W. BENNETT
St. Louis, Mo., U.S.A.

In general, I agree with the objectives of *CA*, but I must admit that in one respect it has not fulfilled my expectations. A number of your articles emphasize extensive and up-to-date bibliographic material. But, working in an underdeveloped country where libraries are very incomplete and where subscriptions to even the best-known foreign periodicals have to be fought for, I do not have access to most of the references cited. If I were the only person working under these circumstances, I would not bring this up; but I think that most anthropologists in underdeveloped countries are experiencing more or less the same difficulties. Because we form only a small minority of the anthropologists in the world, perhaps we should simply resign ourselves to this situation. However, considering the particular importance of anthropology in underdeveloped countries, I venture to suggest that Associates of *CURRENT ANTHROPOLOGY*, when citing important publications to which access is difficult in most of Asia, Africa, and Latin America, should as far as possible give a brief synopsis of the content.

If *CA* really wishes to serve the inter-

ests of anthropologists the world over, it should also help us in some way to keep up with recent international developments and discoveries in our discipline—a rather impossible achievement for us, without outside help. This help will certainly pay off in the long run, because anthropologists who live and work in underdeveloped countries are in a unique position to acquire an intimate knowledge of the local tribal and folk societies, and to collect fresh data about them. INA E. SLAMET-VELSINK
Djakarta, Indonesia

Vide Jean Guiart's suggestion that publication of a corpus of ethnographic specimens housed in museums and private collections would be an important aid to research [January, 1960, p. 1], may I further suggest that *CA* identify the geographical areas whose ethnography and material culture is relatively unknown, and perhaps publish descriptions of one specimen from each such area, in the same manner as these are published in *Man*.

In view of the communications from A. E. Mourant [January, 1960, p. 2] and A. S. Wiener [March, 1960, p. 82], why not ask them and W. S. Boyd to report on the areas of agreement and disagreement in serology? Such a report would be particularly useful, since the extent of divergence in this field is increasing.

Vide Beate R. Salz's letter [January, 1960, p. 2]. I am willing to write an article about India, with or without the collaboration of others, if such a plan for a series of articles on emergent cultures materializes. *CA's* treatment for such articles will be essential.

A. SHARMA
Delhi, India

I suggest that plans for future review articles should be announced in *CA*. In this way, Associates who are interested in providing comments could notify the Editor in advance. I am fully aware of the complications and delays that this procedure will add, but it would help *CA* to avoid one-sided presentations of issues. Of course, the Editor must be free to choose among these prospective commentators.

KARL J. NARR
Weende b. Göttingen, Germany

Neolithic Diffusion Rates¹

by Munro S. Edmonson

THE DEVELOPMENT of a comprehensive theory of culture appears to be dependent upon more rigorous analysis of the communicative processes which are its basis. I should like here to examine the possibility that certain aspects of these processes are considerably simpler than they appear to be, and primarily reflect general relations between time and the communicative space in which culture is located.

Let me begin with a simple model. If we let a circle represent cultural space, then its properties and characteristics generate some useful propositions about cultural communications. Let us assume that the circle is a surface inhabited by a homogeneous field of persons interacting communicatively at some maximal efficiency. In such a field, an invention at the center might be expected to travel in all directions to the circumference at a uniform rate of speed. Other inventions occur-

ring randomly at various points in the circle might also be expected in time to diffuse uniformly throughout the space. The one point in the circle which will have the least mean lag in receiving all of these inventions will, of course, be the center. Let us further assume that some of these inventions stand in an implicative relationship to each other, so that trait *c* is dependent upon the prior conjunction of traits *a* and *b*. It will follow that the earliest possible occurrence of trait *c* will be, on the average, at the center of the circle.

This model would lead us to expect that any single invention of this type would diffuse from its point of origin at a constant rate (measured in sequential persons reached per unit time) throughout cultural space. We should further expect that the probability of at least some types of invention would be a simple inverse function of the distance (in persons) from the center of the circle. In short, communication and invention are predicted by the same variables: time and location in person-space.

I believe this model does not fit cultural reality badly. However, it would be difficult to test it on most ranges of cultural data. I shall argue here that, under certain circumstances, the model can adequately be translated into spatial terms, and that the cultural circumstances of the Neolithic period may reasonably be used to test it. From the hypothetical argument, two relevant propositions can be derived:

1. The apparent diffusion rate, measured by the distance reached by a given trait in a given time, will be essentially constant in Neolithic times, that is, will approximate the real diffusion rate of the model.
2. The apparent diffusion rate will reflect a constant acceleration through time for any one point of origin.

The Neolithic period is a useful test area because it roughly approximated the homogeneous distribution

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Edmonson began his research with a study of differential patterns of humor in the Indian and European population of New Mexico in 1949 and 1950. He has published on North American Indian social organization, Indian and European kinship systems, nativistic movements, and Mexican folklore. He is currently at work on a historical and ethnological study of the Quiché Indians of Guatemala.

The present article, submitted to *CURRENT ANTHROPOLOGY* on April 4, 1960, was sent for C.A. treatment to forty-nine scholars, of whom the following responded with written comments: C. J. Becker, J. M. Cruxent, Fred Eggan, Eugenio Fernández Méndez, Robert Heine-Geldern, A. L. Kroeber, Osmund Menghin, Taryo Obayashi, Charles Parain, Jean Perrot, Merrick Posnansky, Irving Rouse, H. D. Sankalia, Douglas W. Schwartz, A. Petrovský Sichman, Walter W. Taylor, and Dominik Josef Wolfel. Rouse was asked to show the article to Cruxent who was visiting in the U.S.A.; and Kroeber showed his copy of the article to J. B. Griffin. Cruxent's comment is printed after Rouse's and Griffin's after Kroeber's to make clear the references. The comments written for publication are printed in full after the author's text, and are followed by a reply from the author. References cited by a commentator but not by the author are inter-alphabetized with the author's bibliography but distinguished by the initials of the commentator.

¹ Many friends and colleagues have been of material assistance in the preparation of this paper: Drs. Robert Wauchope, Henry Orenstein, Robert Lystad, Arden King, John L. Fischer, Fred Wright, Thomas Ksanes, and E. Lee Hoffman of Tulane University; Dr. Robert F. G. Spier of the University of Missouri; Drs. Clyde Kluckhohn, Paul C. Mangelsdorf, and Gordon R. Willey of Harvard University; and Mr. Thomas A. Lehrer of Cambridge, Massachusetts. I am also indebted to the late Dr. Harlan W. Gilmore of Tulane, whose *Transportation and the Growth of Cities* (1953) was a major influence in directing my thought along these lines.

of interacting individuals hypothesized in the model. The relation of these individuals to geographic space, furthermore, was largely uncomplicated by the major communicational and transportational inventions; hence, linear mileage may be regarded as an index closely associated with "persons." Specialization and social complexity were minimal, at least initially, and hence a large proportion of all communication was horizontal and spatially measurable. And finally, the long distances and extensive time periods of the Neolithic enable us to detect events of an order of magnitude very difficult to measure on smaller scales. The primary difficulties posed by Neolithic history are its inaccessibility to direct observation, and the serious problems of establishing an accurate absolute chronology.

In terms of the known spatial behavior of culture traits, I would doubt that all traits diffuse at the same rate. Accordingly, I have selected traits known to have diffused widely, on the assumption that they would provide maximum apparent diffusion rates for the approximately stable conditions of Neolithic communications. A widely diffused trait may be said to have demonstrated its appeal to human rationality in a variety of cultural circumstances. Thus, my empirically relevant hypothesis is that the apparent diffusion rate of simple, rational Neolithic traits will be approximately constant.

There is, however, an important difference between the model and reality, in that the land surface of the earth is considerably less regular than a circle, and its contours produce apparent irregularities in the distribution or even the sequence of cultural traits, which the model would not. By using the linear distance along the most plausible route of diffusion, however, we can eliminate this effect and reduce it to predictable order.

It may appear contradictory to hypothesize that the diffusion rate will be both constant and accelerating. This comes about because the Neolithic is only a relatively satisfactory field on which to test the theory. We can very well maintain that the real diffusion rate (measured in persons per time) is constant, while the apparent diffusion rate (measured in distance per time) reflects an acceleration factor brought about by man's increasing mastery over space. I believe improved transportation is irrelevant here, except insofar as it provides new routes. The important factor in increasing communication across space is rather the gradual reduction in number of persons required to bridge communicatively a given spatial gap. Thus, it is primarily a matter of the division of labor—of social and cultural specialization. Whereas an early Neolithic trait like pottery had to be communicated, let us suppose, to hundreds of persons in order to travel from Syria to Egypt, a late Neolithic invention like bronze might pass through only a few score individuals socially designated as specialists in, let us say, chalcolithic metallurgy. This process of gradual specialization, shortening the chains of communication in number of persons, but not necessarily in distance, may be expected, then, to create a roughly constant acceleration in the apparent diffusion rate, which causes it to differ irrelevantly from the real rate it is intended to approximate. The two hypotheses to-

gether may be taken to mean that we expect this latter effect to be very small in Neolithic times, and further, that when this source of variation is removed we expect the apparent diffusion rate to approximate closely the real diffusion rate, and therefore to be constant.

In terms of anthropological theory, this hypothesis may be said to rest on a rejection of the concept of "culture complexes," at least for simple, rational traits. Each such trait is interpreted as having its own origin in space and time, and its own subsequently determine history. There is, thus, some disjunction between this proposition and either the *Kulturkreislehre* or the "culture area" conceptions of trait association, with respect to "rational" or widely diffused traits. It is equally a rejection of the evolutionary position, since it relegates the apparently cumulative effects of "evolution" to a position of strict dependence upon cultural communications. The apparent primitiveness or advancement of a given array of traits in a given area is nothing but the predictable loading of trait communication and contingent invention at particular points in an irregular space. Invention and communication, defined by the same variables, simply give priority in time to a central location.

In a certain sense, the concept of a "Neolithic revolution" centering on the ancient Near East also depends upon assumptions of trait association which seem to me incompatible with the evidence. The principle of the "sloping horizon" (Braidwood 1958: 1428) means very different things when it is applied to individual traits and to "complexes." I would consider it likely that many of the traits defined as part of this "revolution" first occurred in or near Iraq; but many others most probably did not. The manifest clustering of these traits in space and time, which lends plausibility to the very conception of a Neolithic age, stage, phase, level, or epoch, may well be a reflection of very little more than the topological properties of the Old World land mass as they are reflected in the communications of a terrestrial but verbal species. Thus, the conjunction of these traits in the Near East is less a reflection of intrinsic functional or evolutionary interrelations among them than of the geographical position of the area in which the conjunction first occurred. Rather than starting in Iraq and diffusing outward, the "Neolithic" appears to a considerable extent to have converged on it.

The model I have proposed would suggest that the apparent creativity of the Sumerians and their neighbors during the later Neolithic follows logically from their early reception of the lore of the earlier Neolithic; and that a similar creativity would later appear elsewhere, as other communicative centers came into possession of a comparable number of early Neolithic ideas. The model would further predict that, far from being a universal stage on either an evolutionary or a diffusionist basis, the Neolithic should be a different event, composed of different traits arriving in a different order and giving rise to different secondary developments, at each of the major centers in which it is represented. I believe this conceptualization fits the known facts more comfortably, and yet more precisely, than a purely historical interpretation or any of the doctrinal construals known to me. These considerations are subject to proof in their own right, but they would make

FIGURE 1

POTTERY

- Reliable dates
- Dubious but acceptable
- Dates rejected or hypothetical

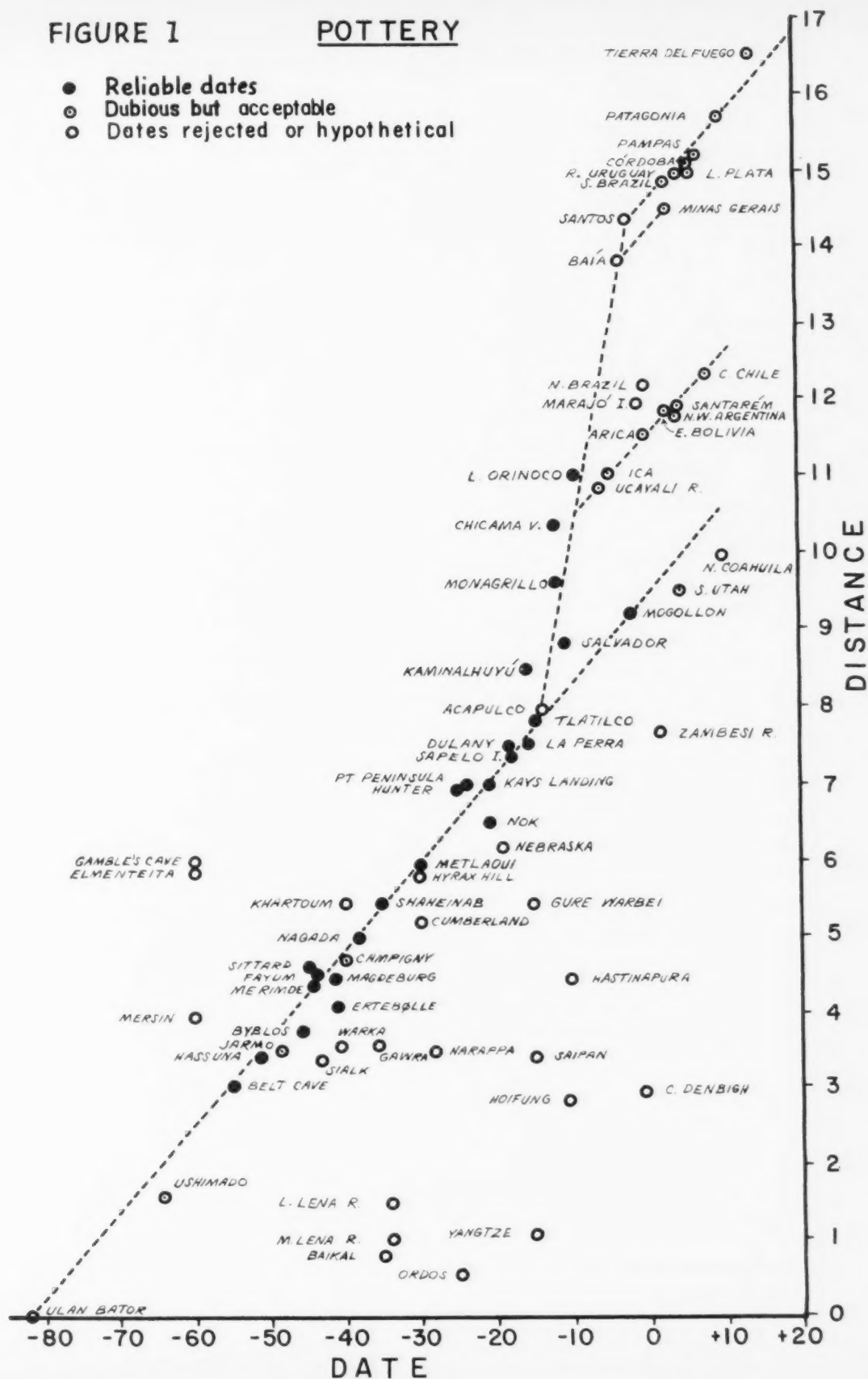


TABLE 1^a
AVERAGE DIFFUSION RATES OF SELECTED CULTURE TRAITS
IN MILES PER YEAR

Miles	Point	Axe	Pot	Crop	Stock	Alphabet	Mean
0-1000	0.10 (2)	—	0.76 (3)	1.00 (1)	1.00 (2)	0.33 (3)	0.62
1-2000	0.03 (2)	—	0.50 (5)	0.67 (2)	0.52 (3)	0.68 (9)	0.48
2-3000	0.21 (1)	1.00 (1)	1.25 (2)	0.84 (1)	1.19 (1)	0.96 (8)	0.73
3-4000	—	1.75 (1)	—	—	—	—	1.75
4-5000	—	4.50 (1)	1.45 (2)	1.00 (1)	—	1.28 (1)	2.06
5-6000	—	—	1.10 (1)	—	—	1.85 (3)	1.47
6-7000	2.60 (1)	0.92 (1)	—	—	—	1.91 (1)	1.81
7-8000	1.00 (1)	2.50 (2)	—	—	—	—	1.75
Mean	0.79	2.13	1.01	0.88	0.90	1.17	
Mean (All Zones)							1.33
Mean (All Traits)							1.15

^aData are from Kroeber 1948. Figures in parentheses indicate number of determinations for a given distance zone. It may be worth remarking that the highest value in the table (4.50 miles per year for the diffusion of the polished stone axe to South Africa by 4000 B.C.) is a dubious datum on the face of it. A date for polished stone in South Africa more congruent with the other implications of this table would be 500 B.C.

a considerably better case if the hypothesis of a constant diffusion rate could be demonstrated.

The evidence I have is not conclusive, but it strongly indicates the essential correctness of the hypothesis. Such examination of Neolithic data from the Old and New Worlds as is readily possible for a nonspecialist generates a mean apparent diffusion rate for the Neolithic of around 1.15 miles per year. Present accuracy of Neolithic chronology does not permit evaluation of the anticipated "evolution effect." Because a thorough assessment by competent specialists is the next logical step, I shall offer my evidence as preliminary confirmation.

My first calculations were based upon a collection of data on six widely distributed traits, ranging in time from Upper Paleolithic to Bronze Age: the projectile point, the ground stone axe, pottery, domesticated plants, domesticated animals, and alphabetic writing. Distance measurements were estimates in airline miles; time was to the nearest century; and points of origin were selected by eye or in accordance with existing opinions. The results are presented in Table 1.

The data in Table 1 are, of course, quite crude. The figures are estimates created by dividing the mean dates of "first occurrence" in each thousand-mile zone into the mid-point distance of the zone. On the other hand, the means for all traits and all zones vary mainly within a single order of magnitude, and the possible sources of error are many:

- (1) Error in the date or place of origin.
- (2) The notorious difficulty of absolute dating in archaeology.
- (3) The crudity of the distance estimates.
- (4) The probability that dates selected are in many cases not really the earliest occurrence, but only the earliest yet discovered.
- (5) The relatively small number of observations.
- (6) The roughness of the calculation itself (e.g., rounding distances to the nearest 500 miles).

I conclude that, despite its ambiguities, the table does not preclude the possibility that the Neolithic diffusion rate approximated a constant in the vicinity of 1.2 miles per year.

TABLE 2^a
POTTERY

Site	Date	Time	Distance	Rate
Old World and North America				
1. Ulan Bator, Mongolia	(-82)	—	—	—
2. Ushimado, Japan	-64	18	16	0.89?
3. Mersin, Turkey	-60	22	39	(1.77)
4. Gamble's Cave, Kenya	(-60)	22	59	(2.68)
5. Elmenteita, Kenya	(-60)	22	59	(2.68)
6. Belt Cave, Iran	-55	27	30	1.11
7. Hassuna, Iraq	-51	31	34	1.10
8. Sialk, Iran	(-43)	39	34	(0.87)
9. Jarmo, Iraq	-48	34	35	1.03?
10. Byblos, Lebanon	-46	36	40	1.11
11. Sittard, Netherlands	-45	37	46	1.24
12. Fayum, Egypt	-44	38	44	1.16
13. Merimde, Egypt	-44	38	44	1.16
14. Magdeburg, Germany	-42	40	44	1.10
15. Warka, Iraq	-41	41	36	(0.88)
16. Ertebolle, Denmark	-41	41	45	1.10
17. Campigny, France	(-40)	42	47	1.12?
18. Khartoum, Sudan	(-40)	42	54	(1.29)
19. Nagada, Egypt	-38	44	50	1.14
20. Gawra, Iraq	-36	46	36	(0.78)
21. Baikal, Siberia	(-35)	47	8	(0.17)
22. Middle Lena, Siberia	(-34)	48	10	(0.21)
23. Lower Lena, Siberia	(-34)	48	15	(0.31)
24. Shaheinab, Sudan	-35	47	54	1.15
25. Cumberland, England	-30	52	52	(1.00)
26. Hyrax Hill, Kenya	(-30)	52	59	(1.13)
27. Metlaoui, Tunisia	-30	52	59	1.13
28. Harappa, Pakistan	-28	54	35	(0.65)
29. Ordos, China	(-25)	57	(6)	(0.11)
30. Hunter, N.Y., U.S.A.	-25	57	69	1.21
31. Pt. Peninsula, N.Y., U.S.A.	-24	58	70	1.21
32. Kays Landing, Tenn., U.S.A.	-21	61	70	1.14
33. Nok, Nigeria	-21	61	65	1.07
34. Valley Co., Neb., U.S.A.	-19	63	62	(0.98)
35. Dulany, Georgia, U.S.A.	-18	64	74	1.16
36. Sapelo I., Georgia, U.S.A.	-18	64	74	1.16
37. LaPerra, Tamaulipas, Mexico	-16	66	75	1.14
38. Kaminaljuyu, Guatemala	-16	66	85	(1.29)
39. Gure Warbei, Somalia	(-15)	67	54	(0.81)
40. Tlatilco, Mexico	-15	67	78	1.16

^aDates are in centuries B.C. (-) or A.D. (+). Time and distance are given in hundreds of years or miles. Rates are in miles per year. All dates other than those obtained by radiocarbon are in parentheses. Distances in parentheses are measured to the center of the region indicated. The same conventions are employed in

POTTERY

I have examined more carefully what data I can find on two archaeologically important traits, pottery and copper, which are prominent in the summary secondary sources and radiocarbon date lists on which I have mainly relied. I should have liked to explore other traits, but the data are not now in a form which facilitates exploration by nonspecialists. Naturally, I am able to get particularly full data on pottery. This is presented in Table 2 and Figures 1 and 2.

My initial expectation was that the data would support separate centers for the diffusion of pottery in the Old World and the New. This assumption created a host of problems and contradictions which fuller data have happily resolved. A single point of origin for the world diffusion of pottery in the vicinity of Ulan Bator,

Mongolia, accounts for most of the earliest pottery in particular regions of the Old World and also predicts the earliest instances of pottery in North America with remarkable accuracy. The same rate predicts the internal diffusion of pottery in South America (from Ecuador to Chile, and from southern Brazil to Tierra del Fuego). It does not predict the appearance of ceramics at excessively early dates in various parts of coastal South and Central America, but this disjunction would be adequately explained by water diffusion at a more rapid rate. I understand this possibility to be seriously entertained by Middle and South American archaeologists on typological grounds, at least as regards a Guatemalan-Ecuadorian connection. The other big gap in the data—the leap from Mongolia to New York—is also rendered

TABLE 2 (continued)

POTTERY

Source	Site	Date	Time	Distance	Rate	Source
(Hypothetical)	41. Yangtze, China	(-15)	67	(11)	(0.16)	(Kroeber 1948)
(Crane & Griffin 1958b)	42. Saipan, Marianas	-15	67	34	(0.51)	(Spochr 1954)
(Braidwood 1958)	43. Hoifung, China	-11	71	28	(0.39)	(Broecker <i>et al.</i> 1956)
(Cole 1954)	44. Hastinapura, India	(-10)	72	44	(0.61)	(Wheeler 1959)
(Cole 1954)	45. San Salvador, Salvador	-11	71	88	1.24	(Wauchope 1954)
(Ralph 1955)	46. Mogollon, N.M., U.S.A.	-2	80	92	1.15	(Krieger 1953)
(Braidwood 1958)	47. Cape Denbigh, Alaska, U.S.A.	-1	81	29	(0.36)	(Collins 1953)
(Childe 1958)	48. Zambesi River, Central Africa	+2	84	77	(0.92)	(Libby 1955)
(Braidwood 1958)	49. Basketmaker, S. Utah	+4	86	(95)	1.10?	(Krieger 1953)
(Braidwood 1958)	50. N. Coahuila, Mexico	+10	92	(99)	(1.08)	(Krieger 1953)
NUCLEAR AMERICA, CARIBBEAN AND BRAZIL (BY WATER)						
(DeVries and Waterbolk 1958)	51. Acapulco, Mexico	(-14)	-	(83)	—	(Hypothetical)
(Braidwood 1958)	52. Monagrillo, Panama	-12	2	16	8.00	(Willey 1958)
(Libby 1955)	53. Chicama Valley, Peru	-12	2	23	11.50	(Wauchope 1954)
(DeVries and Barendsen 1954)	54. L. Orinoco, Venezuela	-9	5	30	6.00	(Willey 1958)
(Braidwood 1958)	55. Marajó Island, Brazil	(-1)	13	39	(3.00)	(Willey 1958)
(Barendsen <i>et al.</i> 1957)	56. Northern Brazil	(0)	14	(42)	(3.00)	(Willey 1958)
INLAND BRAZIL						
(Kroeber 1948)	57. Baia, Brazil	(-3)	-	(138)	—	(Hypothetical)
(Cole 1954)	58. Minas Gerais, Brazil	(+3)	6	7	1.17?	(Willey 1958)
EASTERN ARGENTINA—URUGUAY—SOUTHEASTERN BRAZIL						
(Libby 1955)	59. Santos, Brazil	(-2)	-	143	—	(Hypothetical)
(Libby 1955)	60. Southern Brazil	(+3)	5	(5)	1.00?	(Willey 1958)
(Cole 1954)	61. R. Uruguay-Parana	(+4)	6	6	1.00?	(Willey 1958)
(Kulp <i>et al.</i> 1952)	62. Córdoba, Argentina	(+6)	8	8	1.00?	(Willey 1958)
(Libby 1955)	63. L. Plata, Argentina	(+6)	8	8	1.00?	(Willey 1958)
(Kroeber 1948)	64. Pampas, Argentina	(+7)	9	(9)	1.00?	(Willey 1958)
(Caldwell 1958)	65. Patagonia, Argentina	(+10)	12	(14)	1.17?	(Willey 1958)
(Libby 1955)	66. Tierra del Fuego	(+15)	17	21	1.24	(Willey 1958)
ANDEAN AREA						
(Crane 1956)	67. Huaca Prieta, Peru	(-10)	-	(103)	—	(Hypothetical)
(Barendsen <i>et al.</i> 1957)	68. Ucayali River, Peru	(-6)	4	5	1.25?	(Willey 1958)
(Crane and Griffin 1958b)	69. Ica, Peru	(-4)	6	7	1.17?	(Willey 1958)
(Caldwell 1958)	70. Arica, Chile	(0)	10	12	1.20?	(Willey 1958)
(Wauchope 1954)	71. Lowland Bolivia	(+3)	13	15	1.15?	(Willey 1958)
(Willey 1958)	72. Santarém, Brazil	(+4)	14	15	1.07?	(Willey 1958)
(Cole 1954)	73. Northwest Argentina	(+4)	14	15	1.07?	(Willey 1958)
(Wauchope 1954)	74. Central Chile	(+8)	18	20	1.11?	(Willey 1958)

subsequent tables.

Rates in parentheses are omitted from the calculation of the means. Those followed by a question mark are dubious but acceptable. Distances are measured along plausible routes of diffusion (see Figure 1), using a mechanical map-reader.

Mean: (22 reliable dates) $1.15 \pm .03$ (N = 22)

(19 doubtful dates) $1.08 \pm .08$ (N = 19)

Water rate: 8.50 (N = 3)

more plausible by typology, since the resemblance between northern Woodland and early Baikal pottery has been often remarked. The case for a Mongolian origin is supported by only two radiocarbon dates within 3,000 miles of Ulan Bator; nonetheless, a series of solid dates in the Near East and Africa, Europe, and North America point in that direction with unmistakable clarity.

In compiling the data on the earliest occurrence of pottery in various parts of the world, I have tried to include all the instances of plausibly early pottery I could find. Obviously, some of these sites are more reliably dated or better reported than others. It is my strong impression that the more reliable dates fit the hypothesis better than the dubious ones, and I have therefore attempted to divide the data into three classes: strong, weak, and intermediate. I believe the strongest dates on the list to be numbers 6, 7, 10, 11, 12, 13, 14, 16, 19, 24, 27, 30, 31, 32, 33, 35, 36, 37, 40, 45, 46, 52, 53, 54, and 66. The dates I think are weak enough to be excluded as the earliest instances of pottery in their respective areas are numbers 3, 4, 5, 8, 15, 18, 20, 21, 22, 23, 25, 26, 28, 29, 34, 38, 39, 41, 42, 43, 44, 47, 48, 50, 55, and 56. The remainder are of an intermediate character, dubious perhaps, but acceptable: 2, 9, 17, 49, 58, 60, 61, 62, 63, 64, 65, 68, 69, 70, 71, 72, 73, and 74. Since an assessment of this sort is necessarily complex, but quite material to my results, the basis of these assignments is discussed in greater detail below.

I. The twenty-five most trustworthy dates come largely from areas of intensive archaeological activity. In all cases they appear in sequences of preceramic-ceramic stratigraphy either *in situ* or inferentially within a small area. They are all radiocarbon dated, and in most cases the preceding preceramic cultures are also dated by the radiocarbon method, so that they fit into a relatively tight absolute chronology. In most cases the pottery in question approaches the typological crudity of a "very early" attempt.

(6) The Belt Cave date is the earliest of three closely spaced dates (5100, 5400, and 5500 B.C.), and Belt Cave "software" is immediately preceded by a preceramic stratum dating only 200 years earlier. This dating is loosely corroborated by adjacent finds at Hotu Cave, in which "software" dated 4400 B.C. and preceramic "Subneolithic" 6100 B.C.

(7) Hassuna does not have a preceramic level, but its general placement in Mesopotamian chronology makes its inclusion as an early pottery site reasonable (Childe 1958: 37). It is conceded to be earlier than Warka or Ubaid, and later than Jarmo (which is partially preceramic).

(10) A similar argument can be made for the inclusion of Byblos A village. I can find no clear date for preceramic levels in the immediate vicinity of either site.

(11) The activity of the Groningen laboratory has dated the Neolithic and Bronze Age cultures of the Netherlands profusely. At least six dates at various sites cluster just below the one I have selected for the entry of Danubian pottery (3800, 4100, 4100, 4200, 4400, 4500 B.C.). The preceramic Tjonger (Feddermessaer) culture has been carbon dated as late as 5100 B.C., and the Hamburgian to 5000 B.C. (See De Vries and Barendsen 1954; De Vries and Waterbolk 1958; and De Vries, Barendsen, and Waterbolk 1958.) Menghin suggests that the Feddermessaer and Hamburgian dates, however, may be considerably too late.

(12, 13) The Neolithic levels at Fayum and Merimde have been identically C-14 dated (4100, 4400 B.C.). No direct dates



have yet been reported for preceramic Egypt, but the dating of the later Capsian in North Africa pushes hard on these Egyptian dates.

(14) European scholars have been somewhat skeptical of the early C-14 dates for the Bandkeramik at Magdeburg and elsewhere in Central Europe, but the dates have confirmed each other and there seems to be no reason to reject them. Mesolithic dates elsewhere in Central and Northern Europe are complementary as well.

(16) The dating of Ertebølle (Ellerbeck) "protoneolithic" in Schleswig-Holstein further confirms the new and earlier framework of chronology in Germany and the Netherlands.

(19) The date for Nagada is the oldest of six determinations, and conforms generally to the Egyptian picture as already discussed.

(24) The date for Shaheinab is a shell date, and may be early. Two other determinations give 3100 B.C. In either case, this appears to be a better dating than the estimate for pottery in otherwise Mesolithic Khartoum (4000 B.C.) given by Cole (1951). There are no clear preceramic dates in the Sudan.

(27) The date for pottery in the "Neolithic of Capsian tradition" in Tunisia seems generally plausible. Dates for the preceramic Upper Capsian run as late as 5000 B.C. at Dra-mta el-ma el-Abiod (Kulp *et al.* 1952).

(30, 31) The very early dates for Hunter site and Point Peninsula in New York may be variously interpreted, but the date for the former, at least, seems to be acceptable to specialists. Preceramic dates much later than 2500 B.C. are found at Orient, Long Island (900 and 1000 B.C.; Crane and Griffin 1958a), but the preceramic Lamoka dates seem rather to confirm this dating, since only the latest of them hits 2400 B.C. (Libby 1955).

(32) The Baumer pottery at Kays Landing, Tennessee, is preceded by an Early (and pre-pottery) level dated 2800 B.C.

(33) The figurine and pottery level at Nok, Nigeria, seems reasonably dated, but could easily be somewhat earlier. The "Middle Stone Age" remains that underlie it are dated 3500 B.C. both at Nok and at nearby Zenebi (Barendsen, Decevey, and Gralenski 1957).

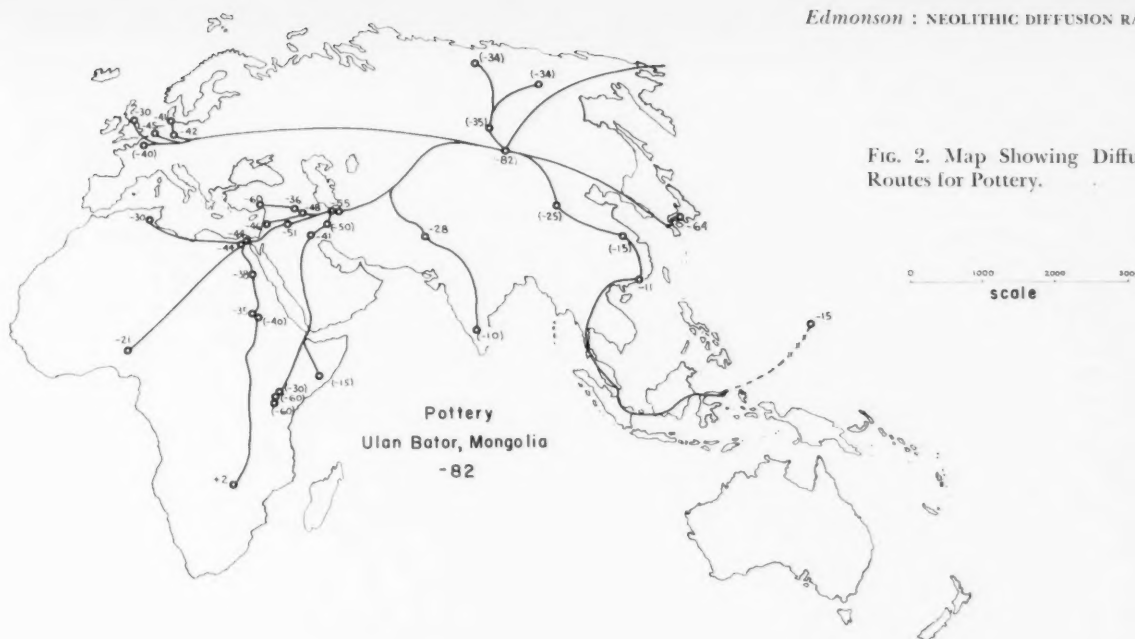


FIG. 2. Map Showing Diffusion Routes for Pottery.

(35, 36) The Sapelo Island and Dulany dates for Florida and Georgia "orange ware" confirm each other. A dubious preceramic date of only 700 B.C. was obtained on a nearby Florida sample (Crane and Griffin 1958a), but the regional pattern shows late Archaic (preceramic) dates of 1900 B.C. in North Carolina (Crane and Griffin 1958b), 2000 B.C. in Kentucky (Libby 1955), and 2300 B.C. in West Virginia (Crane and Griffin 1958b). These seem to confirm the Georgia date as early and authentic.

(37) The Mesa de Guaje pottery from La Perra (and elsewhere) in Tamaulipas fits clearly into one of the longest and best dated stratigraphic sequences yet obtained in the Americas. The latest pre-pottery radiocarbon date in the area is 2000 B.C. (Crane and Griffin 1958a).

(40) The lack of a preceramic-ceramic sequence in central Mexico has been a matter of comment (Wauchope 1954). Nonetheless, the Tlatilco date seems reliable. It is confirmed by an identical date for pottery at Teotihuacán (Libby 1955). A date of 2000 B.C. has been obtained for "apparently preceramic" remains at Yanhuatlán, Oaxaca (Rubin and Alexander 1958).

(45) The Salvadorean Pre-Classical date has no corresponding preceramic, but has been accepted by specialists (Wauchope 1954; Willey 1958).

(46) The arrival of pottery in the intensively excavated Southwest is radiocarbon dated in the Mogollon sequence, but is equally clearly attested in Hohokam and Anasazi stratigraphy, and only slightly less so in the Patayan area. Doubtless the Point of Pines stratigraphy and its twelve C-14 dates will be of material assistance here. Dates as late as 600 B.C. are apparently preceramic in this sequence (Crane and Griffin 1958a; Wise and Shutler 1958).

(52, 53, 54) The dates from Panama, northern Peru, and Venezuela are reasonably reliable, but were not used in calculating a "best rate" because pottery may have reached those areas by water.

(66) The Tierra del Fuego date is the only non-radiocarbon date I have classed as "strong." It is virtually historical.

II. The twenty-six dates I have rejected are not necessarily dismissed because their dating is dubious. Many of them seem very reliably dated, and many others are the result of very careful excavation and good stratigraphy. Most of them are not dated by radiocarbon; some of them were almost certainly late settlements in a general area which had pottery earlier. For various reasons, they fail to carry conviction as demonstrating the date of arrival of pottery in their particular areas.

(3) Originally, I included Mersin (despite the fact that I had read no description of the site) on the strength of its inclusion by Childe (1958) in a list of pottery-bearing sites in the Near East. It is obviously much too early. I have omitted the 5600 B.C. date from Matarrah, Iraq (Braidwood 1958), on the same grounds. To one familiar with the Near East, the dates of the "basal layer" at Mersin and level VI-4 at Matarrah may help date pottery. Since my other Near Eastern dates seem clear enough, I prefer to omit these.

(4, 5) Among the dubious dates, the East African pottery sequence appears to be quite out-of-line. The stratigraphy here is convincing, particularly at Gamble's Cave, and association of pottery with otherwise Mesolithic, or even Paleolithic, stonework seems quite credible. I believe the absolute dates to be far too early. Unfortunately, the chronology in this part of Africa is still floating freely despite a tantalizing scattering of radiocarbon dates here and there in Africa. So far, it seems to me, the beginning of the Gamblian Pluvial may still be thought of as equating roughly with the onset of the Würm Glaciation, but there are a few hints that its end may be substantially later than has been supposed. I see no convincing reason for the Upper Caspian to date any earlier than 6000 B.C. in Kenya, nor any reason that the pottery of its phase D should date earlier than the 3100 B.C. that would be expected. The earliest Caspian date in North Africa so far is 6400 B.C. in Tunisia (Kulp *et al.* 1952). There are no relevant preceramic dates from sub-Saharan Africa.

(8) Kroeber (1948) gives 5000 B.C. as a guess-date for Sialk I; Childe's (1958) estimate of 4300 B.C. was based on the life span of houses of the type built at Sialk. Because the stratum is technically Chalcolithic, I have adopted the later date. It seems unlikely that the assemblage reflects the earliest pottery in this part of Iran.

(15) It is generally accepted that lower Mesopotamia was settled later than the upper valley. Since its stratigraphy begins with a fairly sophisticated pottery, we may assume that Ubaid, Warka, and similar sites do not reflect the earliest possible arrival of ceramics in Iraq.

(18) The pottery at Khartoum is guess-dated. There is no preceramic date.

(20) Tepe Gawra begins with a ceramic level. Since pottery is found substantially earlier at several sites in the immediate vicinity (e.g., Hassuna, and possibly Jarmo and Matarrah), I believe it can be discounted.

(21, 22, 23) The dates cited for the Siberian pottery levels are my extrapolations from Tolstoy's (1958) diagram, on which they are merely indicated as earlier than 3000 B.C., with the Baikal sites slightly earlier than those on the Lena River. In view of the clear sequence established by Okladnikov's excavations in this area, it is particularly frustrating to have no anchor for its absolute dating. My hypothesis would suggest a date as early as 7500 B.C. for the Baikal pottery, and would make it the earliest yet discovered. (According to Menghin^{*} the Choresm pottery is typologically earlier, and primitive enough to be the earliest in Siberia.)

(25) British prehistorians feel that this dating of the Cumberland Neolithic is far too early (Childe 1958: 64-65). For my purposes, it is much too late. In view of Childe's suggestion (1958: 31) of 4000 B.C. for the date of separation of England from the continent, it seems to me that England may be a special case during a crucial period in the diffusion of pottery. Extrapolation from the earliest colonization of Cyprus, and the influence of boats around 4000 B.C. on the diffusion of the Cardial culture in the western Mediterranean (Childe 1958: 47ff.) supports the speculation that an efficient boat may not have reached the English Channel until the later part of the fourth millennium B.C. This would have delayed the entrance of pottery into England to about the date we have. I doubt that the British shock over this date exceeds that of the Germans over setting the Bandkeramik back 1,500 years, or that of Americanists at the early dates of New Mexican maize and New York pottery.

(26) Hyrax Hill pottery has a suitable guess-date for my purposes, but its position in the East African sequence is completely wrong, since it is substantially later than the immediately adjacent Elmenteita and Upper Capsian D pottery. I assume that its absolute date should be correspondingly reduced. (Posnansky^{*} writes that this date can be pushed up to around 1000 B.C. on more recent evidence.)

(28) Despite a scattering of radiocarbon dates in adjacent Baluchistan, one carbon fix on Harappa culture, and the famous seal correlation with Sumer, the Indus Valley's ceramic history remains obscure. There is no local preceramic level. The earliest pottery dates in the Quetta Valley are substantially later (2200 B.C.; Broecker *et al.* 1956); and a *preceramic* date of 3300 B.C. has been obtained at Kili Ghul Mohammed village there (Braidwood 1958). The earliest horizons of pottery in India are beginning to be widely known typologically, but no certain dates are yet available (Wheeler 1959). I do not believe a reliable date for the earliest pottery can yet be extracted from what we have. (Sankalia^{*} writes that several new dates are now available—for Godavari, Navda Toli, Utnoor and Piklihal; some of these fit my argument but I cannot evaluate all of them.)

(29, 41, 43) The long, uncomplicated, ceramic sequences in China may lend some slight plausibility to my argument. The accepted dates do not. The absence of a preceramic-

ceramic continuity, and the almost total lack of a reference point for early Chinese pottery by radiocarbon or any other dating aids, leaves us with no determinate date. The Yangtze date is Kroeber's guess, and hinges importantly on his previous guess for the Ordos. I have included the Hoifung date because it supports his estimate; it is far too late for mine. On the other hand, the Hoifung pottery seems too sophisticated to be "early." There are recent hints that Chinese scholarship may soon be able to fill out this picture; but they are only hints (Woo and Cheboksarov 1959). Meanwhile, we have no date for pottery in China.

(34) The Valley County, Nebraska, date is rejected by the excavator as far too early. It is probably irrelevant to the present argument.

(38) The standard deviation of the Kaminaljuyú Pre-Classic date is 800 years, more than twice as large as that of any other date I have found for pottery. If it is 800 years early, the correction would bring it precisely into line with expectation.

(39) The guess-date for Gure Warbei pottery is my extrapolation from Cole's (1954) approximate chronology. It may have been a bad guess, since it is possible that this is the earliest Somaliland pottery.

(42) The Saipan date is interesting, but may reflect the first settling of the island rather than the first arrival of pottery. In any case, the certainty of long water travel would make its use for the present purpose dubious.

(44) Pottery in Hastinapura and along the Godavari River is guess-dated by Wheeler (1959); but its association with copper in both areas makes it unlikely that it is genuinely early. There is no dated preceramic level in this area. (Sankalia^{*} writes that the Godavari carbon dates are now published; I have not yet seen them. I am embarrassed to have Heine-Geldern^{*} point out that I have misplaced Hastinapura by 1,000 miles; he agrees, however, that it is irrelevant.)

(47) The date of Cape Denbigh pottery is entirely too late to be relevant to the Boreal diffusion of northern Woodland corded ware. It is 800 years too early to be derived from any New World center of independent invention consonant with the pottery dates I have accepted from New York, Georgia, and Tennessee, and with my rate of diffusion. It may be considered relevant, therefore, to the general argument for a single Old World invention of pottery; but in that case it is certainly not the oldest pottery in Alaska.

(48) The Lusu pottery from the Zambesi River has a good radiocarbon date, but no clear position in a sequence.

(50) This guess-date for infiltration of pottery into West Texas and Northern Coahuila is Krieger's (1953). I am not even certain to what part of this rather large area he intended it to apply. (Taylor^{*} writes me that Krieger was actually referring to the Lower Rio Grande area, but that my date is probably close; he would prefer, possibly, a date of A.D. 1200 for Coahuila.)

(55, 56) Willey's (1958) estimates for Marajo Island and Northern Brazil appear to be too late by about 600 years. Since Willey comments specifically on his uncertainty about them, they may well be omitted. I am not sure just where in Northern Brazil he would place the B.C.-A.D. date.

III. The remaining eighteen dates for pottery I can neither definitely support nor clearly reject, though doubtless a better assessment of them can be made by experts. Three are radiocarbon dates; the remainder are estimates more or less based on radiocarbon dates.

(2) The earliest, and unquestionably the most important, single date for pottery is this Japanese determination. Unfortunately, it is a shell date, and therefore possibly too early. It is also the earliest date yet obtained on a Japanese sample

of any type. Although I understand that preceramic remains are now known from Japan, they are not dated; nor, apparently, did they underlie the pottery at this site. The Ushimado date, however, fits the general sequence of Japanese C-14 dates fairly well; and at least two of the latter point toward a much earlier date for the Initial Jomon period than would recently have been considered possible:

INITIAL JOMON:	
Ushimado, Honshu	-61 (Crane and Griffin 1958b)
MIDDLE JOMON:	
Taniguchi,	
Hokkaido	-10 (Rubin and Alexander 1958)
Kamo site	-31 (Crane 1956)
LATE JOMON:	
Ubayama, Honshu	-26 (Libby 1955; two determinations)
Shell mounds	-8 (Crane and Griffin 1958b; three samples)
Uenae, Hokkaido	-13 (Rubin and Alexander 1958)

I do not consider the late Taniguchi date to be a problem, since it is from marginal Hokkaido. The Kamo and Ubayama dates, on the other hand, are both earlier than the guess-date for pottery in China (2500 B.C.), and would force us to question direct derivation of Japanese pottery from China, even without the Ushimado date. They are also typologically late. Since Ushimado pottery is typologically "initial," it seems reasonable to me to accept its date even though its accuracy cannot now be proved. (The hypothetical date for pottery at Ushimado would be 6800 B.C.)

(9) Braidwood's (1958) appraisal of the ten radiocarbon dates from Jarmo leaves that site, and to some degree the Near East generally, in a state of chronological flux. I am unable to determine, from the manner in which they are identified, which of these dates correspond to ceramic, and which to preceramic, levels. Pottery should have reached Jarmo around 5000 B.C. I have arbitrarily selected the first date thereafter: 4800 B.C. The last preceding date is 6000 B.C. This may be plausible, but it is no substitute for fact. The complete series of Jarmo dates is 9300, 9200, 7100, 6900, 6000, 4800, 4700, 4700, 4600, and 3300 B.C.

(17) This date for the Campignian is lifted from Kroeber's (1948) estimate of 5000 to 3000 B.C. It seems to work, but I have no other reason to accept or reject it.

(49) This estimate for Basketmaker acquisition of pottery is in an area where chronology is rather tight and estimates are cautious. I am not certain exactly where in southern Utah it is to apply, but it is about what would be predicted. (58, 60-65, 68-74) The remaining fourteen dates are Willey's (1958) estimates. They all fit my assumptions very well, and I have no reason to question them; but Willey himself expresses many hesitations about several of them.

The mean diffusion rate indicated by the twenty-one best established dates is $1.15 \pm .03$ miles per year. (The Central and South American dates are omitted from this calculation because of probable water diffusion.) A separate calculation of the nineteen "dubious but acceptable" dates gives $1.08 \pm .08$ miles per year. The margins of error here may appear to be impossibly small, unless it is remembered that the rate is determined on a long time span, in which the error of dating decreases as the time values increase. An error of 100 years (or miles) at extreme time and distance values (around 10,000) would yield a rate error of .01. Since the 10,000-year mark on the pottery distribution falls at A.D. 1800, an error of this magnitude at that point is highly unlikely. Given the essentially qualitative selection which necessarily preceded the calculation of this rate, undue importance should not be given to its apparent precision.

In view of the marked departures from homogeneous randomness of at least some of the sources of variability in these calculations—and most notably the radiocarbon error just discussed—I have been tempted to omit any calculation of standard deviations for the mean diffusion rates. I have included them, in the end, only as rough indicators of the range of variability of the selected data. They should not be interpreted as having any clearly-defined meaning in relation to either probability or precision. It may be noted that a single date deviating markedly from expectation can materially change the mean apparent rate with the small number of observations to which we are restricted here.

The remarkable regularity of the rather full data on pottery strongly suggests that the accuracy of radiocarbon dating over much of its range is considerably greater than many of its recent appraisers are willing to admit. If this were not the case, my data should vary more than in fact they do, from dating errors alone. Obviously, the closest consideration must be given to problems of contamination, sampling error, contemporaneity, laboratory methods, controls, and so on, in radiocarbon dating; but on balance the results seem to be quite trustworthy. This general judgment does not, of course, preclude the possibility that any particular date may be very wrong.

Contrary to what might appear from the plot (Figure 2), the selection of a rate for water diffusion of Central and South American pottery was not completely *ad hoc*. The rate was based upon the mean rate of diffusion of the alphabet from Phoenicia to Greece, Carthage, and Rome (4.7 miles per year). A second and closer look at the alphabet convinces me that it is a bad guide, since it appears to have diffused unevenly, with long pauses for reconciliation of the system to new language patterns; it is appreciably more complex than pottery. I have, of course, little theoretical reason for expecting constancy in diffusion by water, which eludes my assumption of homogeneous distribution of persons. As long as navigation takes place in short coastal hops, we might expect some restriction of the diffusion rate through the intervention of an undetermined number of resisting persons. Provisionally, I am content to note that a fairly broad range of rates generally congruent with this assumption is adequate to explain the apparent anomalies in the South American diffusion of pottery. The rate suggested by my data is 8.50 miles per year.

I am informed by Willey (personal communication) that dates of 2100 B.C., and 2400 B.C., respectively, have recently been obtained for the Panamanian Monagrillo and the Ecuadorean Valdivia ceramic strata. Willey also suggests the possibility of independent invention of pottery in South America. The data do not permit a demonstration of this hypothesis as yet, though of course it cannot be discounted. The earlier dates from Panama and Ecuador would obviously contradict the water diffusion hypothesis I have presented, and do not agree with the other Andean dates at my land diffusion rate, though they need not materially affect my hypothesis in eastern South America or north of

Nicaragua. I see no clear way of resolving these difficulties in northern and western South America on present evidence, and so have kept my original construal. It should be viewed skeptically as regards northwestern South America and adjacent parts of Central America, but I do not believe this affects my major argument in any substantial degree.

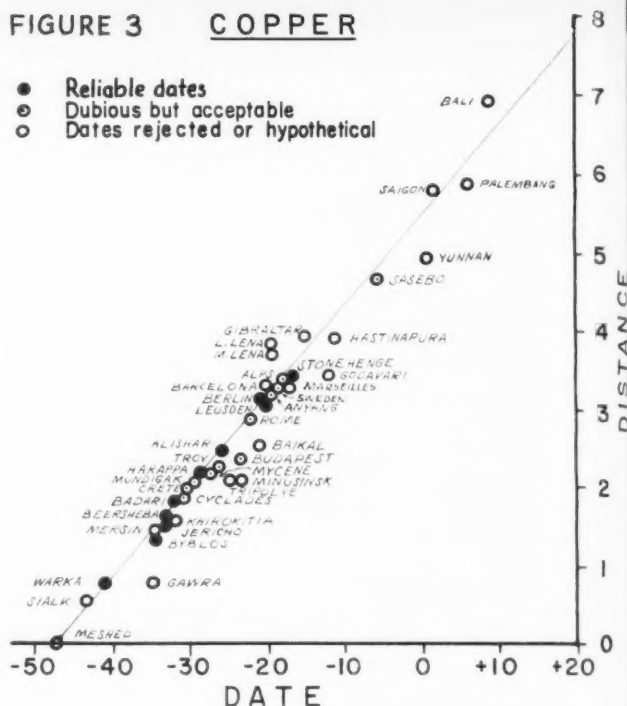
COPPER

Except for pottery, the use of copper and its alloys is perhaps the best documented trait in Old World archaeology, at least for the "Neolithic" period. Nonetheless, the data on copper and bronze require a certain amount of digestion before they can be made to yield a convincing reconstruction of origin and diffusion. As Childe (1958) has observed, there are several differentiable stages in the development of a full Bronze Age metallurgy, and hence the dates for copper and copper alloys tend to reflect different stages of this achievement in different sites and levels. My original intention was to concentrate on the use of tin bronze as a separate and specific trait, but the data specifically relevant to bronze are sufficiently limited that it seemed more appropriate to work towards a reconstruction of the diffusion of copper, using for support as much of the bronze data as seemed plausible.

In Europe and the Near East, the appearance of bronze is almost everywhere preceded by a Chalcolithic or Aneolithic phase of copper-using. This phase is not yet documented for India and the Orient, but there is very little evidence for its absence. Where it can be clearly dated, as at Troy and in Crete, the Netherlands, and possibly Egypt, the duration of the Chalcolithic period appears to be about 300 years. On the other hand, 300 years seems a narrow range into which to squeeze the eventful sequence from Anau IA to Anau III, and it may also be somewhat too narrow for the Halaf-Samarra/Ubaid sequence in central and northern Mesopotamia. After examining the data on copper and bronze, I have concluded that a lag of about 400 years describes the Chalcolithic-Bronze array rather tightly.

It seems unlikely that copper and bronze were invented in exactly the same place 400 years apart. However, there are no clear indications in the data I have found that their origins were far apart. (Contrast the New World distribution, in which one might plausibly assume the two traits to have originated as far apart as the Great Lakes and Bolivia.) Purely theoretically, it seems plausible to expect bronze to appear first in the area which had known copper the longest; and in the Old World there appears to be no indication that this is not the case. Hence, I have adopted the working hypothesis that the two points of origin lie close together spatially, and 400 years apart in time. On this assumption, I have combined the data on copper and bronze into a single table intended to estimate the date of first occurrence of copper in each area by arbitrarily pushing all the bronze dates back 400 years. (These dates are marked *b* in Table 3.) With this procedure, the data are considerably expanded: and the assumption appears to work remarkably well. (Menghin[☆] suggests that a lag of 600 years would be more consonant with the general European data.)

FIGURE 3 COPPER



The combined data on copper and bronze indicate an origin for the use of copper in the vicinity of Meshed, Iran, around 4400 B.C. The data are presented in Table 3 and Figures 3 and 4. Because of its later date, copper is better dated in general than pottery. Even guess-dates are subject to less fluctuation, and more of them can be tied to reliable absolute chronologies through radiocarbon dating or written history. Nonetheless, some of the facts are surer than others, and some detailed comment is necessary about how they were selected and how reliable they appear to be. I believe ten of the dates to be reliable: 2, 4, 5, 6, 7, 13, 16, 22, 23, and 29. Eighteen of them appear to be irrelevant to any tight construal of the rate of diffusion of copper, although many of these offer loose corroboration of the hypothesis: 1, 3, 8, 9, 17, 18, 21, 25, 26, 30, 31, 32, 33, 34, 36, 37, 38, and 39. The remaining eleven are dubious but acceptable: 10, 11, 12, 14, 15, 19, 20, 24, 27, 28, and 35.

I. The ten most reliable dates are all determined by the radiocarbon method. All of them are direct determinations on Chalcolithic cultures, rather than reconstructions based on bronze. All of them are more or less clearly placed in the chronological and cultural stratigraphy of their respective areas. It is not true that they present no problems; but they represent the best data I have found.

(2) The C-14 date for the Ubaid level at Warka seems secure, but could be a reflection of first settlement. I assume this level to be Chalcolithic from its inception, but am not certain.

(4) The date for the "first urban" level at Byblos probably dates copper. I am not certain that it does.

(5) The Jericho (Tell es-Sultan) Chalcolithic was separately dated by the London and Groningen laboratories. Both gave exactly the same reading.

TABLE 3^a

COPPER

Site	Date	Time	Distance	Rate	Source
0. Meshed, Iran	(-47)	—	—	—	(Hypothetical)
1. Sialk, Iran	(-43)	4	6	(1.50)	(Childe 1958)
2. Warka, Iraq	-41	6	8	1.33	(Braidwood 1958)
3. Gawra, Iraq	-34	13	8	(0.62)	(Braidwood 1958)
4. Byblos, Lebanon	-34	13	13	1.00	(Braidwood 1958)
5. Jericho, Jordan	-33	14	15	1.07	(Braidwood 1958)
6. Beersheba, Israel	-33	14	16	1.14	(Rubin and Alexander 1956)
7. Badari, Egypt	-32	15	18	1.20	(DeVries and Barendsen 1954)
8. Mersin, Turkey	(-32)	15	14	(0.93)	(Estimated)
9. Khirakitia, Cyprus	(-32)	15	15	(1.00)	(Estimated)
10. Mundigak, Afghanistan	-30 ^b	17	(20)	1.18?	(Braidwood 1958)
11. Cyclades, Greece	(-30)	17	20	1.18?	(Childe 1958)
12. Crete, Greece	(-30)	17	20	1.18?	(Childe 1958)
13. Harappa, Pakistan	-28	19	22	1.16	(Libby 1955)
14. Mycenae, Greece	(-27)	20	22	1.10?	(Kroeber 1948)
15. Troy, Turkey	(-27)	20	22	1.10?	(Kroeber 1948)
16. Alishar, Turkey	-26	21	25	1.19	(Braidwood 1958)
17. Minusinsk, Siberia	(-24) ^b	23	21	(0.91)	(Estimated)
18. Tripolye, Ukraine	(-24)	23	21	(0.91)	(Kroeber 1948)
19. Budapest, Hungary	(-23) ^b	24	24	1.00?	(Kroeber 1948)
20. Rome, Italy	(-22) ^b	25	29	1.16?	(Kroeber 1948)
21. Baikal, Siberia	(-21) ^b	26	26	(1.00)	(Tolstoy 1958)
22. Leusden, Netherlands	-20	27	31	1.15	(DeVries <i>et al.</i> 1958)
23. Berlin, Schleswig-Holstein	-20	27	31	1.15	(Münnich 1957)
24. Anyang, China	(-19) ^b	28	32	1.14?	(Kroeber 1948)
25. Middle Lena River, Siberia	(-19) ^b	28	37	(1.32)	(Tolstoy 1958)
26. Lower Lena River, Siberia	(-19) ^b	28	38	(1.36)	(Tolstoy 1958)
27. Southern Sweden	(-19) ^b	28	33	1.18?	(Kroeber 1948)
28. Eastern Alps	(-19) ^b	28	33	1.18?	(Childe 1958)
29. Stonehenge, England	-18	29	34	1.17	(Libby 1955)
30. Marseilles, France	(-18)	29	33	(1.14)	(Childe 1958)
31. Barcelona, Spain	(-17)	30	34	(1.13)	(Estimated)
32. Gibraltar, Spain	(-15)	32	39	(1.22)	(Estimated)
33. Godavari River, India	(-14) ^b	33	34	(1.03)	(Wheeler 1959)
34. Hastinapura, India	(-11) ^b	36	(39)	(1.08)	(Wheeler 1959)
35. Sasebo, Kyushu, Japan	(-6) ^b	41	47	1.15?	(Kroeber 1958)
36. Yunnan, China	+ 1 ^b	48	49	(1.02)	(Crane 1956)
37. Champa	(+ 2)	49	58	(1.18)	(Kroeber 1948)
38. Palembang, Sumatra	(+ 7)	54	59	(1.16)	(Kroeber 1948)
39. Java, Indonesia	(+ 9)	56	69	(1.05)	(Kroeber 1948)

Mean: I (10 reliable dates) 1.16 ± .08 (N = 10)

II (11 doubtful dates) 1.14 ± .06 (N = 11)

(6) The dating of the "Ghassulian" village of Beersheba is confirmed by the reliable dating of Jericho. The Chicago laboratory obtained a date of 5500 B.C. for this same level. The Washington reading of 3300 B.C. is obviously better.

(7) Two different determinations on remains from Badari give 3200 B.C. Despite the general expectation of an earlier date, there seems to be no reason to reject them (De Vries *et al.* 1958).

(13) I assume that the tin bronze found in Harappa level remains in the Indus Valley belongs to its later period. Therefore, I accept the initial date of this period as the date for copper.

(16) The radiocarbon date of the Alishar Chalcolithic is generally congruent with Childe's (1958) dating of the Bronze Age in the Aegean.

(22) One determination gives a date of 1900 B.C. for a pre-bronze Netherlands culture (De Vries *et al.* 1958). I do not know whether this was also pre-copper. The earliest copper

^aConventions are identical with those employed in Table 2, except that a small *b* is added here to those dates derived from bronze data by adding 400 years to the age.

date I have accepted fits fairly well with the earliest date for bronze I have found: 1700 B.C. (De Vries and Waterbolk 1958). The earliest bell beakers in the Netherlands are dated to 2000 B.C., and I have accepted this date on Childe's authority: "Everywhere Beaker-folk used copper, albeit sparingly . . ." (Childe 1958: 146). (According to Menghin^{*}, copper is definitely pre-Glockenbecher; he suggests a date of 2200 B.C. for Central Europe.)

(23) I have similarly accepted the Glockenbecher date for Berlin, Schleswig-Holstein, which is identical.

(29) The date of Stonehenge is confirmed by radiocarbon and astronomy. Childe (1958) implies that it is associated with copper, but I am not certain. (Kroeber gives 1700 B.C. as the date of bronze in England.) A date of 2000 B.C. for copper would fit my reconstruction perfectly.

II. Some of the eighteen dates I have rejected are deficient for dating reasons. Others are estimates I have myself drawn up in areas where experts are more cautious. In a few cases, I am not at all sure what relation the date has to copper and bronze. I do not be-

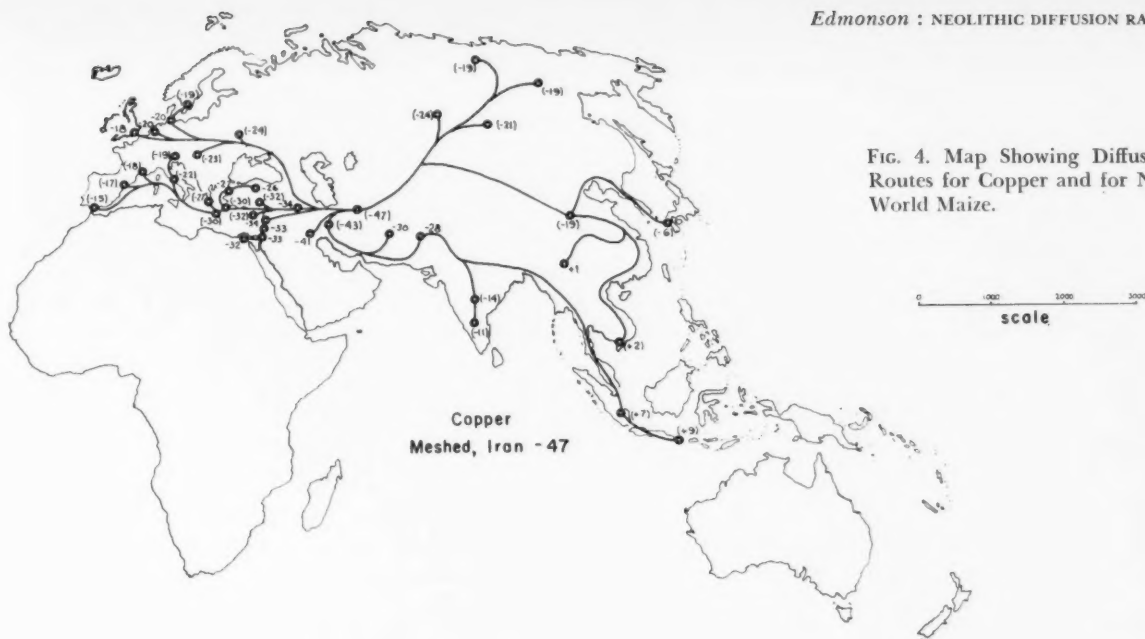


FIG. 4. Map Showing Diffusion Routes for Copper and for New World Maize.

correct in his placement of the origin of sailing vessels as late as 3000 B.C. in Egypt, we may speculate that traits which were diffused later than copper might well reflect the expected acceleration due to long water journeys. On the other hand, the indications are that the spread of copper in Italy and the western Mediterranean was a matter of beach-crawling and island-hopping, rather than of long runs. If this is true, then relatively primitive boats might not appreciably affect the amount of diffusional resistance encountered per mile, at least over long distances.

MAIZE

I should have liked to add further traits from both hemispheres to this compilation, but very few traits are as indestructible or as prominently reported as pottery and copper. Four direct dates on corn cobs from widely separated regions (Table 4) make possible a preliminary reconstruction of the diffusion of maize in the New World. A sketch of this has been added to Figure 3. Because the higher rate values and longer

runs may be assumed to be less subject to error than the lower, I would suppose that a rate of around 1.41 would be the mean diffusion rate for maize on a larger array of data. These four dates yield a mean of 1.38 miles per year.

There would seem to be no reason that the same boats which apparently took pottery to Peru could not have taken maize as well, in which case either maize should appear earlier, or the two traits should appear there simultaneously around 1000 B.C. Mason (1959: 39) appears to hold the latter opinion, but clear evidence of maize in Peru is later than that of pottery. Both Wauchope (1954) and Willey (1958) note a preference for a date earlier than 700 B.C. for the Cupisnique (maize-bearing) horizon in Peru—900 to 1000 B.C. My data suggest a date around 1000 B.C. (rather than the radiocarbon 1200 B.C.) for the earliest north Peruvian pottery, the pre-Cupisnique "Guañape." Pottery must have followed the growing of maize in Mexico by more than a millennium; hence, maize had every chance to precede ceramics in Peru. From the fact that it did not, we are forced to assume either that the development of suitable navigation along the Pacific coast was coincidental with the arrival of pottery at Acapulco; or that maize, for other reasons, stuck to land diffusion. Since the Bat Cave–New York diffusion rate would predict the Cupisnique date in northern Peru perfectly on the assumption of land diffusion, I incline to the second alternative. It is relevant to observe that there is a continuous distribution of maize through the isthmus of Panama, though it is conceivable that there might not have been, since many other traits are not so distributed.

Some of the problems of the origin of maize are typi-

TABLE 4
MAIZE

Site	Date	Distance	Rate	Source
Bat Cave, New Mexico, U.S.A.	—37	—	—	(Mangelsdorf 1954)
La Perra, Tamaulipas, Mexico	—23	18	1.29	(Wauchope 1954)
Middlesex, New York, U.S.A.	—10	38	1.41	(Krieger 1953)
"Cupisnique," Peru	—7	43	1.43	(Willey 1958)

cal of those peculiar to the domestication of all plants and animals. Reliable dates are rare, and almost never are clearly identified at the relevant taxonomic level (species, subspecies, or race). Mangelsdorf (personal communication) informs me that the New Mexican, Central Mexican, and some Peruvian maize (from Ica) is of the Chapalote race, while the La Perra maize is of the Nal-Tel race, "which seems to have had its origin in Guatemala." I do not know to what race the New York specimen belongs. This leaves me with too few dates on any one race to derive a "solution" for maize of any type. For analogous reasons I have had to give up attempted reconstructions for wheat, barley, millet, cotton, squash, pumpkins, and the whole range of domestic animals: present data are simply too scanty. For most culture traits, it is remarkably difficult as yet to get three firm dates of "first occurrence." For plants and animals, the difficulties are compounded by the necessity of clear identification of subspecies.

It is, of course, conceivable that the major invention here at issue is simply the idea of domestication, as has been suggested by several authors. It seems to me that we are close to being able to test this proposition, but careful reconstructions of the history of particular plants and animals will be a prerequisite to such a test. Further data, more carefully handled, are essential. Meanwhile, I have let my reconstruction of maize stand as an example of the problems, rather than as a "solution" for the riddle of maize.

VARIA

In most cases one can do little with trait diffusion unless sufficient data are available to establish a point of origin. This requires a theoretical minimum of three firm dates. I have found a few cases, however, where direct diffusion may well be operative, and which appear to "work" even though I cannot reconstruct origins. These are shown in Table 5. Some of these data may already indicate the passing of the Neolithic conditions upon which my assumptions are contingent. Thus, the flying gallop reached Han China *earlier* than the Siberian occurrence I have noted. The diffusion of iron and the chicken may well have been "neolithic"; tea very probably was not. The implications of the tracing of Hohokam copper bells to the Old Copper culture

of the Great Lakes area may or may not be plausible, but would be consonant with the early use of copper in the Adena culture (800 B.C.), and with its later appearance farther south, including the Mochica adoption of copper in Peru in A.D. 100. The calculation of a rate for the diffusion of the chicken was prompted by my curiosity over whether the cock mentioned in Socrates' last words would not date the chicken better than Kroeber's equally literary dating (based in part on Peter's denial of Christ). From other sources, Kroeber places the chicken in Greece in the seventh century A.D. (Kroeber 1948: 694), which would give a rate of .85. Menghin* (personal communication) tells me that bronze images of chickens in Central Europe date to around A.D. 500, from which I calculate a rate of 1.03.

CONCLUSIONS

Data on the dates of the earliest occurrence of pottery, copper, and maize support the hypothesis that the apparent diffusion rate of "rational" traits under Neolithic conditions approximates a constant, measured in miles per year. Provisionally, therefore, we may conclude that the real diffusion rate—the number of sequential persons reached by a given "rational" trait in a given time—is probably a constant.

There are substantial sources of variation in the measurement of apparent diffusion rates, but the principal sources might well be supposed to be:

- (1) Error in dating the trait occurrence.
- (2) Error in determining the distance travelled.
- (3) Error in the solution for the origin of the trait.
- (4) Indeterminacy of trait acceptance.

Some assessment of these sources of error is appropriate in order to determine the approximate range of variation encompassed in my conclusion. Taken together, these factors are ample to preclude any precise test of the hypothesized acceleration of apparent diffusion with time, which appears to be of an order of magnitude not clearly reflected in data as inexact as we now have.

The standard error of radiocarbon dates is a more or less minimal estimate of the random errors that enter

TABLE 5

VARIA

Trait	Site	Time	Site	Time	Distance	Rate
Copper	Isle Royale, Mich., U.S.A. (Crane 1956)	-18	Snaketown, Ariz., U.S.A. (Kroeber 1948)	+ 9	34	1.26
Cucurbita mixta	Ocampo, Tamaulipas, Mexico (Whitaker <i>et al.</i> 1958)	+ 2	Oraibi, Ariz., U.S.A. (Whitaker <i>et al.</i> 1958)	(+15)	15	1.15
Iron	Northeast Turkey (Kroeber 1948)	(-14)	Entebbe (?), Uganda (Cole 1954)	(+10)	31	1.29
Tea	Peiping (?), China (Kroeber 1958)	- 4	Tokyo (?), Japan (Kroeber 1948)	+ 8	16	1.33
Chicken	Mohenjo-daro, Pakistan (Kroeber 1948)	(-28)	Athens, Greece (Plato)	- 4	29	1.21
Flying gallop	Minoan Crete (Kroeber 1948)	(-18)	Southwest Siberia (Kroeber 1948)	(+ 5)	29	1.26

into dating most prehistoric events. The mean standard error of the 57 radiocarbon dates cited by Braidwood (1958) is ± 308 years. The dates I have selected as reliable give a standard error of estimate in terms of my hypothesis that is rather smaller than this: 221 years for pottery and 89 years for copper. The calculation of mean apparent diffusion rates would tend to minimize the apparent error; and there is good reason to suppose that the calculated rate will always underestimate the true rate, since it is subject to asymmetrical variation through the inevitable inclusion of some "late sites." Somewhat similarly, the standard error of estimate (in years) underestimates the true error, even though it may be a better indicator of the variability of the data than the standard deviation of the rate (in miles per year). The contingent character of the sources of error in my general problem appears to me to counter-indicate the use of powerful statistical techniques to estimate the error, particularly in view of the highly approximate solution of the problem of origin on which the distance measurements depend. Impressionistically, I would suppose that archaeological judgment may reduce the range of normal error in dating over most of the time span here involved well below 300 years, wherever it has good stratigraphy and radiocarbon dates or other absolute chronologies to work from. Equally impressionistically, I conclude that other sources of dating error seem to play a quite minor role in the variability of my data.

Some minor variation is introduced into the distance measurements by simple problems of mechanical accuracy. Two other factors loom very much larger in this connection: the dependence of distance measurements upon the assumed origin, and their dependence upon routing assumptions. The problem of origin is discussed below. The routes I have used are illustrated in the accompanying maps. I see no way of reducing the routing problem to complete determinacy. In the time period under observation, substantial climatic changes, the potentialities of water travel, and possibly demographic considerations are among the many complicating factors. Here again, it is perhaps the general archaeological and ethnographic plausibility of the routes assumed that is germane, rather than a precise statistical estimate of variance in a problem with a very large number of possibly relevant variables. On the whole I have stuck to very simple and obvious routing assumptions; hence I would judge the seemingly small range of variation in the data to confirm the supposition that the distance estimates are a relatively minor source of error in comparison with time estimates.

My solutions for the time and place of origin are only approximate, having been reached by successive approximations in which many of the judgmental considerations already adduced necessarily entered. I believe the origins named are reasonable approximations to "best" solutions for the data I have cited, and I should expect that they would predict fairly well data that I do not have. As solutions, however, they are no better than the data that went into them, and they are less than totally satisfying in that respect. The particular character of the hypothesis on which they rest dictates that given origins which locate particular traits in space and time may efficiently summarize a particular array

of data on the diffusion of each trait, and yet be thousands of miles or years in error when new data come in which change the picture substantially. A reasonably good solution is therefore dependent, in high degree, on the quantity and quality of the data on which it rests. I would suppose that my pottery "solution" is considerably better than that for copper from this standpoint; and although four firm dates are technically enough to solve for and validate an origin (as I have done with maize), this does not inspire much confidence as a basis for predicting all future data about the trait. Obviously, the problem of tracing origins and the problem of determining rates are of a piece in this respect; hence a reliable test of the variability of rates of diffusion will be contingent upon more data on more traits than I can conveniently assemble.

These various considerations lead me to conclude that the macroscopic statistical regularity I am hypothesizing need have but little determinacy at orders of magnitude other than the one I am considering. It appears to hold for cultural phenomena measured in hundreds of miles or years down to the limits of present accuracy of measurement; but these limits appear to be reached as we approach the order of magnitude of the accuracy of radiocarbon time measurements. A combination of archaeological and ethnographic judgment, refined measurements and statistical techniques, fuller data and inter-trait comparisons might well push this limit down to perhaps a century of time and around a hundred miles of distance. I do not see how available knowledge can go farther. Below this order of magnitude, in any case, I should imagine that other factors safely ignored at a gross level might come to be overwhelmingly important. It may be noted that an upper limit for the range of data here considered can be approximated by applying the apparent rate to maximum terrestrial distances along plausible diffusion routes. Even discounting for some acceleration in the rates with time, this limit would fall in the vicinity of 15,000 B.C. or thereabouts, plus whatever additional range is added by the depth of reliable prehistory at the termini.

In general, I have sidestepped the now classical problem in anthropology of what constitutes a trait. For present purposes, of course, I am not under the necessity of counting traits, and hence need not assume them to be irreducible cultural atoms. Nevertheless, it may reasonably be objected that the making of pottery or copper implements, or the growing of maize, are not traits at all, but complexes of traits. The distinction is devoid of consequences in my model, since it can readily be demonstrated that, with a constant diffusion rate, any trait which precedes another at its point of origin will precede it everywhere. The diffusion of pottery would proceed at the same rate whether it is simple or complex, since all the elements necessary to ceramics would either precede the diffusion of pottery or accompany it. Post-Neolithic circumstances materially alter the relevant conditions here, but the model is not intended to cover them.

In all of these calculations I have ignored the possible role of gross population movements. Migration would be a ready enough explanation for anomalies of dating

and distribution; but it seems to me that if migration were a factor of frequent or extensive importance it would create more such anomalies than my data manifest. Obviously, even small population movements would be capable of creating some measure of disturbance in the apparent diffusion rates. Provisionally, I am satisfied that these effects show up less frequently than has sometimes been supposed. A rough estimate of the frequency and magnitude of migrations, based on a tabulation by Toynbee (1934) of major barbarian invasions between 2025 B.C. and A.D. 675, confirms this supposition and justifies the informed guess that the historically documented migrations rarely move faster than 10 to 15 miles per year. An estimate of 18.3 miles per generation is suggested by Wolf (1959: 23) for the peopling of the Americas. Even assuming that the historically known migrations are but a small fraction of all those that have taken place, and bearing in mind the substantial irrelevancy of those which move more or less at right angles to the path of diffusion of a given trait, the disturbing effects of migration might reasonably be expected to be rather occasional in the diffusional history of particular traits. It seems possible, of course, that the scale and speed of such movements are often of such an order that our present crude time measurement does not permit their detection. Close study of this problem in such areas as the ancient Near East, where some such movements are fairly well charted, would be very illuminating.

I should perhaps emphasize that the communications rationale which underlies these propositions is not intended to explain how or why a given trait is resisted or rejected; it may nonetheless turn out to predict fairly well the earliest date at which such a trait could be expected to reach a particular area, if it reaches there at all. Obviously, there are environmental limitations to the diffusion of particular plants or animals. Obviously, urban traits cannot spring up in a sand desert. But these considerations lie outside an inquiry into the flow of information.

If the assumptions underlying this exploration of Neolithic history prove justified, we may expect to find elements of stability underneath the apparent heterogeneity of more modern history as well, but we shall certainly have to work with a space conception more complex than that of surface geography in order to reach them. Old World prehistory and the largely boatless history of the American Indians provide us meanwhile with a thoroughly adequate laboratory for checking these preliminary conclusions.

The problem of water diffusion routes looms much larger and appears much earlier in "Neolithic" data than I would have supposed. Not later than 5000 B.C. in the Mediterranean and 1400 B.C. in Central America, it becomes a factor to be borne constantly in mind. Nonetheless, water travel remains a minor or even negligible factor in many parts of the world well into the Christian era. Thus, many traits continue to diffuse from nonspecialist to nonspecialist in a Neolithic fashion, even in a population in which a great deal of specialized communication is going on. For at least some persons and some culture traits, the world has never been the same since the invention of boats, chariots, cities, and writing created communicative channels

which short-circuit the old person-by-person conquest of space and time. Still, some part of communication remains "Neolithic" in even the most modern urban settings.

The plausibility of a constant diffusion rate raises important questions about how such constancy could come about. I believe it means that, beyond a certain threshold frequency of contact, additional interaction does nothing to speed up the average rate at which simple and rational novelty will be accepted by each individual it encounters in its travels. We are familiar with the resistances of individuals, for example, in acculturation situations. While I am prepared to believe that some individuals have a considerably greater flexibility in this respect than others, I should not be surprised if this faded into insignificance in the long communications chains of the millennial events we are here considering. The finding suggests a rather low but vital level of communicative efficiency—possibly "rationality" would describe it well—which is statistically constant in the human animal. It suggests, too, that this capacity is limited by an equally constant force operating against it.

It seems likely to me that such a force is precisely what is represented in the systematizing and configurative dimensions of cultural reality, and psychologically in the various security operations of ethnocentrism. I should expect this force, too, to be related to the fundamental properties of human communication. On the face of it, the centripetal and self-systematizing tendencies that are so salient a feature in restricting the diffusion of basic vocabulary elements (Swadesh 1952) would suggest this possibility. It seems reasonable to suppose that any cultural system which is highly structured (kinship, for example) can be made so and kept so only by a higher degree of selection of available innovations than seems to characterize the acceptance of rational and technological inventions. It seems altogether likely, too, that the relatively casual contact which is sufficient to diffuse independently assorting traits or simple systems would be totally inadequate for the transmission of complex systems or patterns. The latter might be expected to require the relatively intense indoctrination provided by family life or similar institutional contexts, and hence to flow mainly in communicational channels that are vertical and generational rather than horizontal and spatial. This would explain the apparent conservatism of language, folklore, kinship, and some kinds of ethnic traditions. Measured in persons, modern man is much closer to the Neolithic generationally than spatially.

Men apparently need systems, and must perforce pay the price of this requirement in communicative efficiency. It may be that various types of culture traits diffuse from person to person at different rates depending upon their involvement in this need, and that their speed of diffusion will tend to lie somewhere between the moderate velocity of rational diffusion and the much slower rate allowed by basic vocabulary replacement.

Much light could be shed on these problems by investigation of the range and variability of apparent diffusion rates for traits of different types under Neolithic conditions.

Comments

By C. J. BECKER*

The whole idea of this article seems out-of-line with contemporary methods of archaeological research along broad lines. One could find this method used in archaeology some fifty years ago, when the distribution of one detail of pottery decoration, for instance, might be traced throughout Europe by scholars who gave no attention to culture complexes, nor even considered different pottery styles as unities. Today, there are many pitfalls in studying a single element without considering the different complexes of which it may be part, and in which its value to the whole civilization may vary widely. Pottery does not have the same significance in hunting and in farming cultures, nor in village and urban societies.

Many of the objections that could be raised against the present paper are in fact mentioned by Edmonson himself as possible sources of error. I am thinking, for instance, of the fact that it is quite impossible to neglect the geographical and ecological background in studying the spread of neolithic cultures. Today we can follow, in broad lines, the development of the first neolithic cultures in central and northern Europe. The Danubian cultures must have spread very rapidly through the whole of Central Europe, from the Ukraine to Belgium. But after that it was nearly a thousand years before food-producing cultures, with the aid of a new technique, and carried by new peoples, penetrated southern Scandinavia. And it was perhaps more than two thousand years later that a civilization based on farming could colonize northern Scandinavia (or parts of it). But a fact of such importance has escaped the author, partly because he uses only Carbon-14 dates (which so far are much too few in this part of the world, and in many instances are not generally accepted); and partly, it seems, because he is not aware of the different uses of the term "Neolithic." In Central Europe and southern Scandinavia, the Neolithic period begins with the first food-producing cultures, which in some—but not all—cases introduced pottery; but in the woodland zone of Scandinavia, Russia, and Siberia, the Neolithic period begins with the first use of pottery by peoples who in every other way still had a "mesolithic" way of life.

A very limited number of "dated" sites with early pottery can, of course, give an impression that pottery, as a "neolithic" culture element, spread with a constant rate of diffusion. But such a conclusion seems absolutely impossible to a scholar with first-hand

knowledge of at least some of the European material.

By FRED EGGAN*

I find Edmonson's paper on Neolithic Diffusion Rates very interesting. The model for diffusion which he sets up—a homogeneous field of persons in which "an invention at the center might be expected to travel in all directions to the circumference at a uniform rate of speed"—is reminiscent of Wissler's (1926) "law of diffusion," though Edmonson's conception of the "center" is more sophisticated than Wissler's.

Edmonson believes his simplified model fits "cultural reality" rather well, and he tests it out on a number of neolithic traits whose distributions can be mapped and dated, in part at least. For these he finds that "the Neolithic diffusion rate approximates a constant in the vicinity of 1.2 miles per year."

All this sounds reasonable until we turn to the maps and discussion. For pottery, Figure 2 reconstructs a pattern of distribution originating in the vicinity of Ulan Bator, Mongolia, around 8200 B.C. Edmonson had originally expected to find separate centers for the diffusion of pottery, but this assumption "created a host of problems and contradictions. . . . A single point of origin for the world diffusion of pottery in the vicinity of Ulan Bator, Mongolia, accounts for most of the earliest pottery in the particular regions of the Old World and also predicts the earliest instances of pottery in North America with remarkable accuracy" (my italics). While this extrapolation to Central Asia may give a pleasing symmetry to time and space, it in no sense accounts for the earliest pottery in the Old World or predicts the earliest instances in the New. Nor does the Bering Strait date bridge the two continents.

For copper and bronze (Figure 3), the New World data clearly contradict the assumptions of the author, and are omitted from the discussion. But if the radiocarbon dates for the Old Copper culture are reasonably correct, Edmonson would have to derive the Old World developments from the New World discoveries.

The distribution of maize in the New World is interpreted, in terms of a few preliminary radiocarbon dates, as centering on Bat Cave, New Mexico. No archaeologist, to my knowledge, thinks that Bat Cave represents the place where maize was first domesticated, despite the long sequence of development there indicated. In my opinion, the Bat Cave dates are too early; and should be run again. But granting their validity for the moment, it apparently took

Edmonson : NEOLITHIC DIFFUSION RATES

maize longer to diffuse to the neighboring Tularosa Cave than to distant Tamaulipas. Here we had better wait for a few more dates.

Since in my opinion Edmonson has simplified his model beyond "cultural reality," a discussion of his assumptions in detail is perhaps not necessary. Cultural anthropology has been continually searching for simplifications of the cultural processes which will enable it to handle the complex masses of data that have accumulated. A rejection of the concept of "cultural complexes" and an appeal to "human rationality" have been tried before and found wanting. Edmonson's more sophisticated model does not fit "cultural reality" adequately enough to convince me that it is a useful formulation.

By EUGENIO FERNÁNDEZ MÉNDEZ*

Edmonson's concern with a comprehensive theory of culture is highly commendable. We can indeed agree with him that a sound theory of culture depends largely upon more rigorous analysis of communicative processes (be they learning, invention and discovery, or diffusion); and, furthermore, that these processes reflect primarily "general relations between time and space." As Edward Sapir (1949: 392) once said, regarding time perspective in culture history: "Our problem may be metaphorically defined as the translation of a two dimensional photographic picture of reality into a three dimensional picture which lies back of it."

A scientific theory of culture will certainly have to be based, also, on the establishment of reliable regularities (laws, whenever possible) in communication, i.e., processes of acculturation or diffusion. The demonstration of cultural regularities regarding the constancy of diffusion rates, within Neolithic, pre-Neolithic or post-Neolithic times, would indeed be a methodological advance in the solution of reasonable approximations to the problem of origins, which has been for long a *noli me tangere* of anthropological research. In the case of "rational traits," the apparent diffusion rates and the real diffusion rates of the model posited by Edmonson do approximate; but, chain and single assumptions in the argument are many, since: (a) "Neolithic history is inaccessible to observation," and similarity is taken as evidence of connection by diffusion; and (b) distance and time estimates are in many cases "notoriously difficult": therefore, irregularities (errors in dating, errors in the solution for the origin of a trait, differential water diffusion rates in Central and South America, possible occasional role

of migration, etc.) are logically, but truly, explained away. In any case, Edmonson's careful and discriminating handling of the particular evidence is impressive, and the apparent constancy of diffusion rates is most challenging.

There is, however, one important theoretical point with which I should like to take issue. The assumption leading to the rejection of the concept of functional trait association (if that is what is meant by "culture complex"), as well as of "the evolutionary position," does indeed debilitate the main argument. It is not at all evident—as Edmonson largely takes for granted—that the concept of the "Neolithic Revolution," which does depend upon the assumption of functional interdependence of traits, could be shown to be "incompatible with the evidence." Earlier, in formulating his diffusion model, Edmonson himself has been forced to declare that some "inventions stand in an implicative relationship to each other, so that trait *c* is dependent upon the prior conjunction of traits *a* and *b*."

Moreover, evolution, as Edmonson seems to presuppose in his argument, is not at all contradicted by the idea that culture may diffuse as single traits, clusters, or complexes. The evolutionary process—as distinct from history—is basically an orderly (i.e., necessary) sequence of forms which presuppose "the patterned conjunction of traits" irrespective of particular time and place. Therefore, I cannot logically see how Edmonson is led "in terms of anthropological theory" to reject the concept of culture complex, and thus to a further rejection of the evolutionary position. Regarding these specific points, the postulated contradictions are logical *non-sequiturs*.

Even if, in accordance with the proposed hypothetical model, we theoretically accept Edmonson's conceptualization of different traits arriving in a different order and giving rise to different secondary developments, this still does not, by definition, invalidate the concept of the "Neolithic Revolution." This would simply be a case whereby diffusion brings technological and social features to all societies, thus convergently re-creating the required patterns (Childe 1951: 16). In short, I think that clarification of the implied and very special meaning of *evolution* and *Neolithic* in Edmonson's theorizing would be in order.

Edmonson is on much safer ground when he offers the evidence on Neolithic diffusion rates for pottery and copper as preliminary confirmation of his primary contention that *the apparent diffusion rate for rational traits approximates a constant of around 1.15 miles per year*. His own critical assess-

ment of the limitations of his evidence is indeed plausible. He accepts the origins named as "reasonable approximations to best solutions." His claim that the communication rationale underlying his propositions could serve to predict probable goal-diffusion-dates, is based on such compounded presuppositions that we could expect, at least, some deviations from the expectations. These would probably increase as we extrapolate them into more modern history, when many more variables intervene in the communicative processes, and the proportional scale of events would magnify the irregularities.

As to the points raised towards the end of the paper, regarding constancy of diffusion rates as related to "the systematizing and configurative dimensions of cultural reality" and "the various security operations of ethnocentrism psychologically," they are indeed quite aptly taken. Due cognizance is given to the impact of rational and technological inventions upon structured cultural systems, as well as to the sociological and other factors that condition the acceptance or rejection of diffused traits. As J. H. Steward (1955: 21) has thoughtfully reminded us: "There are problems in a theory of culture in which man's rational and emotional potentials are not a zero factor in the equation."

In summary, Edmonson has made an interesting contribution in method and approach to the study of statistical regularities in culture, within rather strictly limited circumstances: i.e., Neolithic diffusion rates. But, I think that his claims regarding more general theoretical aspects (i.e., functionalism and evolution) are less rigorously taken than the importance of the subject would demand.

By ROBERT HEINE-GELDERN

Most of us will probably agree with Edmonson's rejection of the old evolutionary theories and of the *Kulturkreislehre* (both practically dead), and with his emphasis on the necessity of studying the distribution and diffusion of single culture traits. He goes too far, however, in rejecting the concept of culture complexes. After all, the possession of stone axes does not make a culture "Neolithic," nor would anyone call the Andamanese a Neolithic people because they make pots. The concept of the Neolithic presupposes a whole complex of traits, not always the same, and admittedly difficult to define. It implies a particular mental attitude, different from both that of hunters and of peoples with higher civilization. Any archaeologist familiar with the prehistory of the ancient Near East will probably come to the conclusion that

it was not merely the diffusion of single traits, but even more the spreading of whole complexes, which resulted in the creation of the oldest Neolithic cultures in this area.

Edmonson's statement that "the apparent diffusion rate will reflect a constant acceleration through time for any one point of origin" sounds enigmatic. Even the explanation he later gives does not make the sense quite clear, and is, moreover, questionable. He points out that the manufacture of pottery, for instance, had to be shown "to hundreds of persons in order to travel from Syria to Egypt," whereas "a late Neolithic invention like bronze might pass through only a few score minds of individuals socially designated as specialists in metallurgy." Would metallurgy really have spread faster than pottery to tribes of hunters and food-gatherers, comparable to the present-day Australians or Bushmen? It would have been more correct to say that metallurgy might have spread faster than pottery in those instances where it was introduced into areas where the ground had been prepared for its acceptance by the previous establishment of neolithic cultures.

The author stresses repeatedly the fact that many of the dates listed in his paper are dubious and that the number of really reliable dates is limited. Actually, there are vast regions from which we have no C-14 dates at all. Was it justified, under these conditions, to build such an ambitious scheme on such shaky foundations?

Moreover, the lists of dates also contain a good number of inaccuracies, errors, and arbitrary or even abstruse interpretations for which the author himself is responsible:

(1) He bases his date for the first appearance of copper in Yunnan, A.D. 100, on a bronze Buddha image in the Freer Gallery in Washington, radiocarbon dated with the help of a piece of wood found in its interior as around A.D. 500. Since Edmonson had come to the conclusion that in the Near East and in Europe the appearance of copper antedated that of bronze by about four centuries, he subtracts four hundred years from the date of the Buddha image and gives A.D. 100 as that of the first use of copper in Yunnan. Why a Buddha image should mark the beginning of the use of bronze in this area is beyond comprehension. It has, of course, nothing whatsoever to do with it. Moreover, the author's arbitrary assumptions that the use of bronze must everywhere have been preceded by that of copper, and that their initial dates were always 400 years apart, is totally unwarranted. He says in a note that his Yunnan date is "dubi-

ous." It is more than that; it is worthless. Actually, a magnificent Bronze Age culture flourished in Yunnan half a millennium prior to Edmonson's copper date, and it is almost certain that it was not preceded by the use of pure copper. There certainly was never a Copper Age in northern China, as the author believes, dating its beginning around 1900 B.C.

(2) A C-14 date for Harappa, 2800 B.C., is listed twice and attributed to the second edition of Libby's *Radio-carbon Dating*. No such date is to be found in Libby's book. The author may, perhaps, have found it in some other source. Incidentally, the date would be meaningful only if it refers to the lowest (i.e., pre-Harappan) level at Harappa.

(3) All this is surpassed by what the author says concerning Southeast Asia. As dates for the first appearance of copper he lists for Champa A.D. 200, for "Palembang, Sumatra," A.D. 700, and for "Java, Indonesia" A.D. 900, referring to Kroeber's *Anthropology* as his source. Needless to say, Kroeber is innocent of this. Edmonson's note on these dates is so revealing that it must be quoted verbatim:

The dates given by Kroeber (1948) for the Champa kingdom in Annam, Sri-Vijaya in Sumatra, and Boro-budur in Java have been included as possibly relevant late Chalcolithic cultures. The expectation [according to the author's theory concerning the rate of diffusion through time and space] does not work out badly, but the history of copper and bronze in these areas is still highly uncertain.

The author obviously does not know that at the dates indicated, Champa, Srivijaya and Java were fully historic Hindu-Buddhist kingdoms in which iron, as well as bronze, was in general use. By A.D. 200, Champa must have known bronze for at least half a millennium. In A.D. 700 and 900, the use of bronze in Sumatra and Java must have been at least 1000 years old. Furthermore, in none of the three areas was bronze preceded by pure copper.

I doubt that any archaeologist will agree with the author that "The implications of the tracing of Hohokam copper bells to the Old Copper culture of the Great Lakes area may or may not be plausible." This is not in the least plausible; the Mexican origin of the Hohokam type cannot be doubted. The derivation of the South American use of copper (Mochica) from the Old Copper culture of the Great Lakes region is, of course, utterly impossible. The ancient Indians would have had to use airplanes to travel from the Great Lakes or from Adena to Peru without leaving any traces in the intervening areas. It has been well established that knowledge of copper reached Mexico

very late, perhaps around A.D. 900, and without any doubt came from South America.

It was necessary to cite these examples to demonstrate how the author handles some of his data. They would, in themselves, suffice to shatter whatever confidence we might have had in his results. Dwelling on further details of this kind would be superfluous, since the author's whole theory and the principles on which it is based are unacceptable. His own lists of "possible sources of error" and of "substantial sources of variation in the measurement of apparent diffusion rates" should have convinced him of the futility of his attempt to work out a general rule.

The author starts his thesis by assuming a circular model which, he thinks, "would lead us to expect that any single invention . . . would diffuse from its point of origin at a constant rate." No account is taken of the fact, discussed many years ago by Dixon, Goldenweiser, Kroeber, Lowie, Norden-skiöld, and others, that peoples react to new traits in completely different ways, and that the acceptance or rejection of a given trait is unpredictable. Nor were traits always diffused radially; evidence to the contrary abounds. The author believes that his model "does not fit cultural reality badly." It does not fit it at all.

The most serious objection one can raise against the article's assumptions concerns, of course, the fact, well established by archaeology, that Neolithic culture traits were not always diffused merely from village to village and from tribe to neighboring tribe, but in many instances by real migrations. In this manner, a trait could, within decades or even years, spread over distances which it would have taken centuries to traverse by slow diffusion from neighbor to neighbor.

Edmonson states expressly that in all his calculations he "ignored the possible role of gross population movements." In order to minimize the importance of such movements he cites Toynbee's tabulation of major barbarian invasions between 2025 B.C. and A.D. 675, which, he thinks, "justifies the informed guess that the historically documented migrations rarely moved faster than 10 to 15 miles per year." It would be easy to cite numerous examples to the contrary, but since the migrations in question are not comparable to those of early Neolithic tribes, this would not serve any useful purpose. Edmonson furthermore refers to Eric Wolf's estimate of 18.3 miles per generation for the peopling of the Americas. This is again beside the point, since whatever

value we may accord to Wolf's estimate, the American continent was first peopled by hunters and food-gatherers, and not by Neolithic agriculturists.

It is not difficult to show how greatly Edmonson underestimates the importance of population movements as a factor which may invalidate his calculations. Leaving aside those instances in which archaeological evidence seems to indicate fast-moving migrations, we need only cite the historically documented migrations of South American tribes with Neolithic cultures, particularly those of the Tupi-Guarani, which covered enormous distances within years. One of these groups, starting from the coast of Brazil and voyaging part of the way up the Amazon River, took only ten years to reach Moyabamba in northern Peru in A.D. 1549. This means an average of at least 220 miles a year. One can imagine how much faster the group might have travelled going downstream.

There seems to be a widespread notion that, in the early Neolithic, peoples must have moved very slowly. The contrary is true. In many instances, rapid exhaustion of the soil due to primitive agricultural methods must have provided a constant incitement to move on. An even greater incitement must have been the limitless availability of land, thinly populated by hunters and food-gatherers who were unable to withstand intruders. This means that we shall have to reckon with relatively fast-moving migrations, particularly in the early periods.

Occasionally Edmonson seems to be disturbed by doubts ("The contingent character of the sources of error in my general problem appears to me to counterindicate the use of powerful statistical techniques to estimate the error, etc."), but he "impressionistically" dispels these doubts. His statement that in order to establish the point of origin of a culture trait one needs a minimum of three reliable dates—which obviously means that it can be done on the basis of three dates—testifies to an optimism which will hardly be shared by many of his colleagues.

The way the author applies his method in order to determine the area where pottery originated cannot fail to surprise and shock archaeologists. By plotting the dates for the supposed earliest appearance of pottery at various sites on a map and by linking them together with lines he comes to the conclusion that pottery must have been invented where all his lines converge, i.e., at Ulan Bator in Mongolia. He has no fact to go upon, but nevertheless

assigns to the invention of pottery at Ulan Bator the "hypothetical" date of 8200 B.C., and says:

The case for a Mongolian origin is supported by only two radiocarbon dates within 3000 miles of Ulan Bator; nonetheless, a series of solid dates in the Near East and Africa, Europe and North America point in that direction with unmistakable clarity.

Any comment would be superfluous.

To judge by the map, the two radiocarbon dates within 3000 miles of Ulan Bator are those from the Belt Cave in northern Iran, 5500 B.C., and from Ushimado in Japan, 6400 B.C. The latter is based on shell, and therefore is unreliable and certainly too early. The author is well aware of this and says so. Therefore he should not have used the date at all; but it obviously fitted his hypothesis.

The time needed for diffusion over a given distance, be it of whole Neolithic complexes or of single traits, was necessarily dependent on so many different factors—both geographical (forests, mountains, streams, deserts, soil, climate, etc.) and human (acceptance or rejection, initiative of leading persons or inertia, diffusion from group to group or migration, etc.)—and on so many unknown imponderables that any assumption of its having proceeded at a uniform rate would from the very first appear unreasonable. This applies even to single cases. A trait spreading from Jericho, let us say, to Vladivostok might have taken 500 years to cover the first 1000 miles; then, carried by migrations or under other particularly favorable circumstances, 50 years for the next 1000 miles; and, slowing down again, 1500 years for all the rest of the way. Therefore, no useful purpose can be served by dividing the full number of miles by that of years (necessarily uncertain), thus computing a completely meaningless average. Attempts to determine on such a basis the places of origin of any culture traits are, of course, void of any value.

By A. L. KROEBER*

At first touch, a constant diffusion rate seems preposterous. Next, when I glance at "Table 1, Average Diffusion Rates of Selected Culture Traits, In Miles per Year," and the footnote to this title says: "Data from Kroeber, 1948," I feel a pang at having been caught out at something improper. Certainly the rounded and rerounded means of other people's estimates in a work of reference for the general reader were not intended to be put to such precision use. Defensively, I turned at once to my China-Japan transmission list on page 744, where 19 items are lined up to show that they varied all

the way from 100 to 2089 years to travel from North China to central Japan. Edmonson cites from this very list: tea took from -4 to +8, or 11 elapsed, centuries to travel this shortish distance of 1600 miles "from Peiping (?) to Tokyo (?)," at the average rate of 1.33 miles per year as he figures 12 centuries. I would prefer Si-an to Nara, or somewhat over 2000 miles in 1100 years, or 1.82. The mean transmission time for the 19 traits is just 800 years, which yields a rate of 2.0 for his distance, of 2.5 for mine.

In Edmonson's Table 1, the diffusion rate for six widely-distributed traits is calculated at 1.15 miles a year; in Table 2, for neolithic pottery, it is again 1.15; for pottery, in Table 3, it comes out as $1.16 \pm .08$; and for maize, 1.38. Of course, the model is not intended for use on particular years or a specific year. Say the rate approximates 1100-1200 miles in 1000 years, and it is much "truer." The approach is wholly macroscopic.

The diffusion rate for pottery (or of course any other trait), obviously depends on when and where the invention was made. Edmonson assumes the vicinity of Ulan Bator in Outer Mongolia at -82 centuries—a bit over 10,000 years ago. This is, of course, not a C-14 date, nor is there any direct evidence whatever that pottery was made in Mongolia either in -8200, nor for that matter earlier than anywhere else in the world. Edmonson calls the specific time and place "hypothetical." What does this mean? It means that to operate with his method he needs a starting point definite in time and space. Excavation and the laboratory not having provided one, he assumes one, makes it up, so he can test his hypothesis as to constancy of diffusion rate. How does he make it up? Evidently by looking over a map of the world, and seeing about when and where he would have to have his diffusions start in order to reach Iran, Egypt, and northwest Europe by the earliest pottery dates actually established for the west, and Japan and North America eastward.

Yet not all is harmony and corroboration. Mersin, Gamble's Cave, and Elementeita, in Cilicia and Kenya, all at -60 centuries, are out of step—too early. But, somewhat farther west and considerably later we have Egyptian datings of -44 for pottery at Fayum and Merimde, and Nagada -38, and beyond in the Sudan at Khartum and Shaheinab at -40 and -35, with Tunisia and perhaps Kenya at -30. In Europe, Netherlands, Magdeburg, Ertebölle are about the same distance from Mongolia, and their dates prove to be about the same: -45, -42, -41, with Cumberland in England not quite

1000 miles farther and a bit over ten centuries later: -30. It was a bit of a jolt to me to find that authentically dated pottery had appeared in north-west Europe as early as on the Nile; but if radiocarbon tests make the two areas coeval on this point, we just have to accept it; especially as there are *pre-ceramic* dates of -51 and -50 for Tjonger and Hamburgian in north-west Europe.

Mixed in are some very "soft" dates, like no. 17 of -40 for Campignian, which "is lifted from Kroeber's (1948) estimate of 5000-3000 B.C." I know nothing first-hand at all about the Campignian, of course, and the estimate of the period is nothing but my compromise guess at reconciling other people's opinions. I am pleased that my guess "seems to work," but I am appalled at its being brought into consideration alongside C-14 dates. They might confirm it; but it cannot strengthen them. In fact it is not even a date, but the mean of an era.

East of Ulan Bator, but still in Asia, we strike trouble once more. The oldest C-14 date for a ceramic site anywhere is -64 for Ushimado on Honshu in Japan, as published by Crane and Griffin. Without knowing anything specific about Japan, the site, or the circumstances, I refuse to accept this date, and challenge it as a presumptive error in urgent need of laboratory rechecking. Edmonson points out that "hypothetical date" for Ushimado would be even older, -68. This merely means that, granted his starting point at Ulan in -82, and his generic diffusion rate of 1.15, the 1600 miles from Ulan to Ushimado call for only 14 centuries of travel time instead of 18 at the computed rate of 0.89 mile per year. But the decisive fact is that the oldest other carbon-validated date for pottery in Japan is Middle Jomon at Kamo, -31, which is corroborated by -26 for late Jomon at Ubayama. For initial Jomon at Ushimado, -40 might be an expectable and acceptable date; but the 3300-year lapse from -64 is so extreme as to throw full burden of proof on whoever wants to maintain it. I would lay 3 to 1 that when a reauthenticated date for Ushimado pottery is available it will be much short of 6400 B.C., and probably well on the short side of -5000.

Chinese ceramic guess dates are later than those for Japan. I agree that the hitherto assumed derivation of Japanese pottery from China may have to be abandoned, possibly even reversed. But until we get some hard dates, the whole question had better be kept in suspense. Edmonson's interesting general theory at present seems to fit China-Japan badly. Let us admit that, and concentrate on trying to get reliable

time facts. His theory may prove genuinely useful as an over-all one even if certain particular areas continue to fail to conform to it.

The next dates that are surely relevant are a long distance from hypothetical Ulan Bator—between 6000 to 7500 miles away via Bering Strait—in New York (Hunter and Point Peninsula), Tennessee, Nebraska, and Georgia (Dulany, Sapelo), and clustered—in that order—from -25 to -18, or 4400 to 3700 years ago. They are the earliest radiocarbon dates associated with pottery in the Western hemisphere, in Edmonson's list—earlier than anything in the American Southwest, Mexico-Guatemala, or South America; and there are a half-dozen of them.

There also are, of course, a lot more carbon ballots to be counted, but unless the returns from the southern two-thirds of the Americas begin pretty soon to include some that are firm in being close to 5000 years old, it looks as if Edmonson's present sample poll would approximately predict the ultimate vote of the facts.

This means, first, that the United States had pottery before Latin America had it; second, that it probably had it, not because of greater inventiveness, but because of greater nearness to Asia where the invention was made; third, that the old suggestion of a genetic connection between Eastern Woodland and Siberian pottery was probably true; and fourth, that this means more than Asian influences being responsible for a component in eastern U.S.A. culture, and that the Asian origin was probably responsible for most or all pottery in our hemisphere.

Of course, a single site anywhere south of the Rio Grande with repeatedly confirmed carbon dates of 3000 B.C. or older will knock all this inferential construct into a collapsed heap. But until such a date is found and confirmed—or unless it already exists and Edmonson has overlooked it—we must accept his construal as much the most likely in the present state of knowledge.

The derivation of all Latin American pottery from the U.S.A. Woodland will shock many of us, because within less than the apparent thousand years after Latin America had got its first pottery, several areas in it had far outstripped the Woodland in total culture—in religion and art as well as in ceramics and sculpture—and continued to develop thereafter while the Woodland stood nearly static.

There is a close parallel here to Holland and Magdeburg's having pottery as early as Fayum and Merimde, though fifteen centuries later Egypt had a state, temples, pyramids, taxes, metals and at

least one city, whereas northwest Europe had nothing at all comparable. We must evidently be ready not to be overimpressed by the later pictures of Egypt and Mexico-Peru, which loom so much fuller in our knowledge and which we therefore tend to project back into a long past. If future dates corroborate Edmonson's presently available ones, the great Near Eastern and Nuclear American civilizations had a startlingly rapid rise. It looks as if Nuclear America was nuclear only since 1500-1000 B.C.; before that, it was marginal in features like pottery, abreast in some others. Edmonson has remarked something similar to this about Sumerian creativity.

The implied backwash of pottery and advancing culture from Mexico into the U.S. Southwest is not too hard to accept; nor is the subsequence of Andean ceramic culture to Mexican, especially as speeded up by a water travel jump. A good many items of fact have indeed been pointing these ways for some time back. We have simply been conservative about departing too drastically from the customary projection back into a remote past of the dominance and time precedence of the more advanced American culture wholes.

Edmonson has newly redemonstrated one point of method; that while breadth of vision ultimately pays off, it is difficult to balance in one's own mind many factors at once. It is wise to select one factor or trait, to limit attention to it rigorously, then follow it through ruthlessly and see where it leads us. Even if new dates drastically alter his present inferences, it is clear that we shall never return wholly to the interpretation that was prevalent before.

Another moral: In the last analysis, it is fuller control of facts that determines what view is the right one, not who has the subtlest mind, the greatest gift of theorizing. The idea of carbon dating was a bright one, but it was almost bound to occur to a nuclear physicist, and not to an archaeologist, though it is archaeology that has so far been the chief gainer. But for anthropology, C-14 age determinations are just plain brute facts of building stones. Yet it is they that have enabled Edmonson to put his hypothesis to a test. And when we shall have more intercorroborative carbon dates concerned with crucial early ceramics in strategic regions, we shall have a more evolved and better confirmed interpretation of what actually happened. Let us have facts, new facts, more facts, relevant to almost anything, and I at least will cheerfully wait for the superior theorist to show us new uses and relevances of the facts.

He will be along—probably sooner, now there are so many of us who are attentive. Science, they say, is organized knowledge; but it is equally true that only organized knowledge can breed science.

A standardized diffusion rate of cultural innovations is much like a standardized decay or attrition rate of morphemes. They both deal with change and assume that the rate of change can be measured, and that it is operationally profitable to assume that it is a constant, and see if available measurable phenomena confirm the assumption; or if they do not confirm too well, we can at least ascertain how far actual change varies from a constant, and under what impinging circumstances it varies which way. Impinging circumstances means factors outside the phenomenon; the constant is at first assumed to be of internal origin. As we control more organized knowledge on the problem, the internal factor may prove to be not simple but composite, and some of its components may turn out to be also external, though secondarily and seemingly internal. The situation has by then become complicated; but we have positive knowledge of more elements in the complex than we had before; until finally we achieve a breakthrough to a new consideration or level, which simplifies things once more.

Glottochronology operates on the automatic or spontaneous systems we call languages. It follows one and the same language system through, measuring its change in time. (It is becoming clear that there are complications according to position in space also, which must be taken into account. That means we know more and are making progress.) Edmonson isolates a very small system like pottery—mostly it would be called a trait complex, or unit element—and follows it through both time and space to ascertain its diffusion rate; with the interesting by-product of probably upsetting some prevalent archaeological taken-for-granted. I doubt whether he will get a real honest-to-God constant, an impermeable, unmodifiable one. For one thing, he is already admitting a probable different diffusion rate by water travel; and he has mountain ranges and deserts and dense forests still to reckon with. No matter; his 1150 miles in 1000 years as of 1960 is an achievement. It is a fine base from which to take off for further exploration. Just so, Swadesh's 81% of cognate retention per 1000 years, amended a few years later to 86%, has been exceedingly productive: it has led us to organizing genetic classifications of historic language groups which previ-

ously we stopped inquiring into as soon as it had become clear that there was genetic connection between their constituent members; not to mention other results.

Now Gordon Hewes points out that in 1850 Latham announced that the average rate at which languages change was capable of being approximated. Pretty far-sighted; and conservatively wise also in its "average rate" and "approximated." Yet it was just about a century before Swadesh made the attempt to determine what the average rate was; for that is what glottochronology seeks, or ought to seek, along with particular departures from the average for particular reasons in particular cases.

I think the same holds for Edmonson's average diffusion rate.

I expect, too, that this his first try has yielded a figure that will approximate pretty closely to what the grand or ideal average will prove to be when we shall control several times as many reasonably firm fact dates as now. To put it another way, the ideal rate will be a useful model from which to work toward particular specific situations.

Finally, while I doubt whether what constitutes a culture trait is really a "classical" or legitimately established problem in anthropology, I concur with Edmonson's "side-stepping" the problem. As he says, the distinction between trait and complex is devoid of consequence for his model, pottery diffusion proceeding at the same rate whether it is the one or the other. To which I would add that pottery is sufficiently defined for satisfactory study by its specific physico-chemical properties, its processes of manufacture and uses, its distribution in time and space and association with other parts of culture, and that defining it by putting it into the category of a trait or a complex is scientifically not much more than a verbal irrelevance, or a belated survival. Really satisfactory definitions are an end-product of research accomplished, not a precondition of achieving it.

By J. B. Griffin

Professor Kroeber, with both maturity and judgment, has commented on Edmonson's "Neolithic Diffusion Rates" with more complacency than I can. This paper is an interesting example of intellectual agility and statistical sleight of hand. In regard to the diffusion of "copper" it should be noted that Edmonson is primarily talking about bronze; and that neither the societal structure which enabled bronze to diffuse, nor the societies in which it became securely seated, are "Neolithic." It may justly be argued that Edmonson is often not dealing with the "Neo-

lithic" in any reasonable sense of the term; nor with "diffusion"—and if the latter is so, then "rates" hardly enters into the picture. I do not feel capable of commenting on some of the traits or the sites at which they occur. My knowledge of some of the data on which my comments are offered is only second, third or fourth hand, which makes me somewhat uneasy.

Copper or bronze will be dealt with first because it will take less time. Kroeber's estimate of -19 centuries for Anyang needs to be raised to -14; if I understand current interpretations. Tolstoy's dates for the Baikal and the Lena Valley are derived from Okladnikov; some other authors would make them 600 to 1000 years later.

I wonder why Edmonson did not carry out his computations for the spread of copper-bronze to the New World from the Old World—or vice versa, as the radiocarbon dates (-55, -36) from Oconto, Wisconsin suggest. The two North American dates he cites in this paper are -18 from Isle Royale, (presumably an Old Copper Culture pit which I watched being excavated) and +9 for the Snaketown copper bells. Edmonson argues that the presence of copper in Adena at -8 strengthens a diffusion connection between Snaketown and Isle Royale (or Old Copper). And that in some way the Mochica adoption of copper at +1 enters into this north to south diffusion rate. Perhaps this is one of the examples where the "communicative processes" are not as simple as are needed for Edmonson's model. It seems perfectly clear to most archaeologists in the Eastern United States that the Old Copper Culture was concentrated between 3000 to 1000 B.C., and that there is no sound reason to believe that it was derived by either direct or stimulus diffusion from Northeast Asia. It is also believed that the Adena-Hopewell-Mississippi use of copper had its origin in the Old Copper tradition, but that the uses to which the copper was put were profoundly modified. For quite a few years, no one with knowledge of North and Middle America has suggested that eastern U.S. copper was a source for Mexican or South American metallurgy. For quite a few years, the evidence on the Southwestern U.S. cast copper items is that they were derived from western Mexico, where the technique seems to have developed about A.D. 700-900 as a result of diffusion of some kind from northwestern South America. As a concluding remark on copper, I may say that there has been a real lack of communication between Edmonson and his advisors if Old Copper can be included as a part of "Neolithic Diffusion Rates."

While Kroeber's comments indicate

shock at a date of -64 for Ushimado pottery and a betting disbelief in its antiquity, he has just learned from me the still more shocking dating for Jomon pottery. I am shocked that Edmonson in his blind use of dates and words did not start his pottery lines from Honshu; and in support of my contention that he should have, I offer the University of Michigan laboratory's recent dates for Earliest Jomon at the Natsushima site near Tokyo. These are ca. -72, and make it easily the oldest radiocarbon dated pottery in the world. I did not say the oldest pottery in the world, even though it is typologically a rather simple fictile fabric. I would agree with Edmonson that the attributed age of Kenya pottery is too early, but I would wait for a radiocarbon date rather than use Edmonson's line.

The dates in the Near East, Mediterranean, and Europe in his alignment make some reasonable sense because this is a known diffusion area, and because many generations of archaeologists have worked to present the evidence for the development and spread of many varieties of pottery along varying diffusion and migration routes. Why Edmonson's technique was not applied to the known or assumed spread of pottery traditions or complexes is somewhat of a mystery.

The dates for the Baikal at -35 are Tolstoy's, taken from Okladnikov; but I doubt that either source has dates of -34 for the middle and lower Lena. Other writers, besides myself, have been skeptical of these dates for Siberia in terms of what seemed to be known about eastern Asiatic pottery. The Ordos and Yangtze dates are guesses, and not very modern ones at that.

The date for the earliest appearance of pottery in China, and more specifically in north China, is not known. Certainly neither the painted pottery nor the black pottery cultures of about 2000 B.C. and 1700 B.C. have anything to do with it. The utilitarian pottery throughout north China is a cord-marked (not fabric or basketry) earthenware that is markedly different from early Near Eastern-Mediterranean pottery, and significantly different from the central southern and southwestern Siberian pottery which came in from the west. It must have had a considerable antiquity in China before 2000 B.C. It is linked in its essential ceramic features with Earliest Jomon, with the earliest known pottery in the Fukien-Hongkong area of southeast China, and with the spread of pottery into Malaya.

The Norton Complex at the Iyatayet site at Cape Denbigh is not the earliest pottery in Alaska, but the Choris pottery is not much earlier—back, say, to -7. Both Choris and the Norton Com-

plex are derived from the lower Lena, and the linear and check stamp surfaces of this Alaska pottery are ultimately derived from North China, not earlier (assumed) than about 1700 B.C. when such surface finishes began to be common in the Yellow River drainage and as far east as Shantung. We will assume with Edmonson that the -1 date for Denbigh area pottery is too late; but not for his reasons.

Edmonson has accepted the -24 date for pottery on a site in western New York dated by Libby. Ritchie in his report on the site clearly pointed out that the pottery was not in direct association and that the evidence was presumptive. He has since, however, used this date as though it dated the pottery. Edmonson has also cited Caldwell for a -25 date for the Hunter site in New York. Actually, these two dates are the same radiocarbon run on the same specimen from the same site. Furthermore, the solid carbon date obtained by Libby is probably wrong, whether it has anything to do with the pottery or not. The Orient culture of Long Island has the same general behavior patterns as those of Ritchie's Point Peninsula I people at the Hunter site. They were communicating. Furthermore, similar cultural practices are known from New England to the western Great Lakes where they are dated from 1500 B.C. to 500 B.C. Most of these local representatives of the late Archaic and Transitional complexes are without pottery. Edmonson has grossly misused the archaeological data available for New York when he cites Lamoka as having a bottom date of -24, to support the arrival of pottery at -24. The much longer lived and varied Laurentian Culture is known to occur between Lamoka and Point Peninsula I. Besides, -24 is not the bottom date for Lamoka, but is instead the best dating for Lamoka in Central New York.

The Sapelo Island and Dulany dates in Georgia are not identical, they are not the same "ware" and neither is Orange ware. The dates for fiber tempered pottery in the far southeast at about -20 precede by about 1000 years any firm date we have for pottery in the north. In addition, the fiber tempered pottery is a unique ceramic complex and its origin is difficult to determine. At one time I proposed its origin as the result of stimulus diffusion from Woodland pottery people in the north; but at the moment this seems less likely than it did 15 years ago.

Middle American and northwest South American pottery is as distinct from Woodland pottery as is Jomon from Jarmo. Middle America is the same kind of diffusion area as is the Near East-Mediterranean. The dates used by Edmonson for Tlatilco and

Kaminaljuyu are both too early—the first by about 700 to 500 years, and the second by at least 1000 years. Better dates on both sites have appeared; and the ceramic complexes at neither site should have been seriously accepted as being as old as the first radiocarbon dates suggested. Neither complex had anything to do with early pottery in Meso-America, for they were too developed and sophisticated, and were known to be preceded by simpler pottery.

Before Edmonson has a chance to incorporate Willey's personal communication of -24 centuries for Ecuadorean Valdivia, may I suggest a bit of caution, in that the ceramic style to which Valdivia belongs should not be anywhere near that early; and the dates were obtained, I hear, from shell specimens. Mona-Monagrillo may be a "reliable" date, for the pottery does not belong to the late pre-Classic styles.

With the data he chose to use, why did Edmonson not spread pottery from New York to the Southwest? He could have used the convenient Valley, Nebraska, date at -19; but he chose to ignore this on the advice of the excavator—which was sound judgment by the excavator and by Edmonson. Curiously, he chose to ignore Mersin and Matarrah because they did not fit. The application of this approach by the author is not model science.

I did not think that anyone with any knowledge of American prehistory, or of maize, could seriously postulate or imply the diffusion of maize from Bat Cave, New Mexico. Many American archaeologists have been very carefully avoiding a -37 date for Bat Cave maize. Their reasons are not entirely clear to me, but I respect their judgment. On the bases of other early dates in the southwest on maize that is equally primitive, the Bat Cave date seems to be about 1500 to 2000 years too early. The -10 date for Middlesex, New York, cited from Krieger, is based on one kernel from a mixed site with late Point Peninsula on top, from which the kernel fragment probably came. Ritchie stopped mentioning maize in Middlesex about 15 years ago. It should not be planted there again. Is it too much to suggest to Edmonson that, when he seriously wants to measure the spread of maize in a "Neolithic" age, stage, phase, level, or epoch, he first try to measure its spread in pre-ceramic societies which is what it certainly did? Furthermore, it might be measured in its spread north and south from an area of the world where it had a reasonable chance of having been developed.

Among statements that I find quite

baffling, I may cite the following. "Pottery must have followed the growing of maize in Mexico by more than a millennium; hence maize had every chance to precede ceramics in Peru. From the fact that it did not, we are forced to assume either that the development of suitable navigation along the Pacific Coast was coincidental with the arrival of pottery in Acapulco or that maize, for other reasons, stuck to land diffusion." Perhaps it is that word "Acapulco" that throws my thinking astray. "Acapulco" has many meanings but the only pottery that I can think of which could reasonably have come to that bay by boat are the Chinese ceramics carried from the Far East by the Spanish galleons. Maize did precede pottery in Meso-America by from 2000 to 1500 years, but was apparently too deeply rooted to move at Edmonson's diffusion rate so that the Peruvians had a pot to boil it in when it got there.

By OSMUND MENGHIN*

Edmonson's model is not universally applicable in the temporal sense. For instance, nobody would think in such terms of conditions in Protolithic (Lower Paleolithic) times. Even its application to the higher hunters and gatherers of the Miolithic (Upper Paleolithic), whose conditions were anthropologically, socially, and economically so much closer to those of the Neolithic, seems neither possible nor plausible. On the other hand, by fully metal-using periods, craft specialization had turned the communicative process into other channels, and at the same time lifted it to another intellectual plane.

Further, the model is not universally applicable in the spatial sense, either, as shown by Edmonson's difficulties when taking water-travel into account, particularly in regard to trans-Pacific relations. This is only one example showing that environmental circumstances, which are not always uniform and constant, cannot be neglected in long-range calculations, especially since the diffusion of culture traits and culture complexes is not only a temporal-spatial communicative process.

Last, but not least, this "communicative process" is an essentially human activity. It can have different aspects while in process, which react on the temporal-spatial sequence. People may or may not accept a cultural trait or idea presented to them, as shown by the absence of copper and bronze in prehistoric Africa, for instance. Decisions by human beings, whether conscious or unconscious, made voluntarily or sometimes involuntarily, stand behind the

cultural facts we unearth. Thus the diffusion of cultural traits and complexes is an essentially historical process, whose complex causal connexions can be illustrated by a model and discussed in abstract terms, but can not be abstracted in a universally valid formula. And in very few instances do we have enough facts to understand fully the causal relations of prehistoric events.

Thus, Edmonson's model can necessarily be applied only to Neolithic conditions, and this only tentatively. Setting aside the reliability of the dates Edmonson uses in applying his model to the diffusion of Neolithic ceramics, I am of the opinion that no Old World prehistorians, or other specialists in this field, will accept his placing the center for the world diffusion of pottery in Mongolia, or the date of about 8000 B.C. he assigns to it, on such scanty and subjectively evaluated evidence. I think I have made clear above the reasons why such an aberrant result is possible after quite sound theorizing. Therefore, I am not prepared to accept Edmonson's mean rate of diffusion, even as a guide; it cannot be generalized, and is of only purely theoretical value. If the application of Edmonson's model on his best material of 73 dates gives such a dubious result, I do not think it is necessary to discuss his other examples (although his results for the diffusion of copper seem quite reasonable, if instead of Meshed, Iran, we assume the Near East area for its place of origin).

I do not think that the problem at the basis of this paper can be solved practicably by such reasoning, nor perhaps at the present state of our knowledge; it is of exceptional importance for future research. I also think that this paper is a fine example of American anti-historic thought—the belief that statistics can compensate for comprehension.

By TARYO OBAYASHI*

I find Edmonson's attempt very stimulating, if not quite convincing. I have no *a priori* objection to the idea that any single invention of a certain type would, under certain circumstances, diffuse from its point of origin at a constant rate. However, as Edmonson admits, this kind of study surely involves many difficulties. Those involved in dating the first occurrence of a trait, in determining the distance it travelled, and in assuming what route it took are particularly apparent in his treatment of pottery and copper in East and Southeast Asia. In the following, I shall discuss them:

* I appreciate the aid of Professors Ichiro Yahata, Takeshi Sekino, and of Mr. Hakari, of the University of Tokyo, in drafting this comment.

POTTERY (Table 2)

Date 2. Ushimado, Japan -64

The oldest C-14 date known for Jomon pottery comes from Natsushima shell mound, near Yokosuka: 7280 B.C. \pm 500 (shell), and 7490 B.C. \pm 400 (charcoal) (Serizawa 1960: 102). Whether the diffusion route was via Korea is still uncertain.

Date 29. Ordos, China (-25)

We do not yet know exactly when pottery first appeared in North China, but a date of about 2500 B.C. in southern Shansi or western Honan has been suggested (personal communication of Prof. Sekino; cf. Mizuno 1955: 172). If the first pottery in this area was painted ware, the diffusion route probably was from Eastern Europe to Northern China. But it also seems possible that combed pottery in Ordos goes back to 3000 B.C. In this case, it was diffused from the north, probably Siberia (cf. Loehr 1952: 51).

Date 41. Yangtze, China (-15)

The first occurrence of pottery in the Yangtze region is more obscure than in Northern China, but indications seem to suggest a date somewhat earlier than 1500 B.C. (cf. Mizuno 1957: 31-32).

COPPER: (Table 3)

Date 24. Anyang, China (-19)b

Edmonson's assumption that copper preceded bronze everywhere has not been proven for China, where bronze seems to have been the oldest metal. It occurred about the 16th or 17th century B.C. (personal communication of Prof. Sekino, cf. Mizuno 1958: 11).

Date 35. Sasebo, Kyushu, Japan (-6)b

The priority of copper to bronze in Japan has not been proved. Bronze objects appear in large quantity about the beginning of the Christian era (Mori 1960: 78); but the first occurrence of bronze in northern Kyushu is assumed to date from about 200 B.C. (personal communication of Prof. Yahata).

Date 37. Champa (+2)

Palembang, Sumatra (+7)

Java, Indonesia (+9)

Date 38. Palembang, Sumatra (+7)

Date 39. Java, Indonesia (+9)

The priority of copper to bronze in these areas has again not been proved. The oldest metal age in Southeast Asia is thought to be the Dongson culture, with bronze objects. The dates of first occurrence, and the diffusion routes given in Figure 3, are not acceptable. According to Heine-Geldern (1951: fig. 37), bronze entered Southeast Asia, particularly Indochina, via Central Asia and along the Sino-Tibetan border, in the 8th century B.C.

Edmonson does not treat the diffusion rate of bronze, but of copper. Therefore, the dates of the East-Asiatic and Southeast-Asiatic occurrences discussed above should be omitted from the calculation of the diffusion rate. I would be very much interested to see if the hypothetical Neolithic diffusion rate will be strengthened after coordinating comments by other colleagues.

By CHARLES PARAIN*

Edmonson's article undoubtedly opens a new avenue of research by which all concerned with problems of

cultural diffusion will profit. His conclusions will force them to reflect on the elements of these problems with more rigor and to ask themselves very useful questions, even if they do not completely agree with the method proposed.

Specialists will probably challenge the dates of most ancient appearance of this or that trait. One can admit, however, that the apparent rates of diffusion of the traits on which the argument is based constitute provisionally usable approximations, and these rates do roughly appear to be sufficiently close to one another.

Nevertheless, one may question the significance and bearing to be attributed to these observations, as well as the validity of the analyses which explain them, especially with respect to diffusions realized after the Neolithic epoch, but in the words of the author, in a Neolithic fashion. As the author considers that some part of communication remains "Neolithic" even in the most modern urban settings, we have a right to examine the conditions of these recent diffusions more closely, not only for themselves, but for the light that they can and must cast on the most ancient diffusions. The analyses would become completely valid only if they conformed to observations made on the whole historical development, at least in so far as the means of massive diffusion, from the printing press to the radio, do not intervene.

In a general way, it seems to me that the author proceeds from conceptions which are too mechanistic, and limits himself to explanations which are too mechanistic. It seems indispensable always to keep in mind the qualitative obstacles besides the quantitative obstacles; and transmission by leaps beside linear transmission.

(1) Distance is not the only factor in the diffusion of a culture trait from one civilization area to the whole of another civilization area. It is indispensable that the cultural level of the host people or the general conditions of its cultural development permit the acceptance and the assimilation of the trait in question. For centuries, Pharaonic Egypt remained impervious to the adoption of the wheel, which nevertheless was well-known to her Asiatic neighbors and partners, and this continued until she learned to utilize the war chariot. It will be noticed here that because of the vital interest involved in their adoption, military techniques, or techniques considered under their military aspect, are the ones best transmitted in spite of differences in general technical level. Transmission of a technique, at a uniform speed over immense areas, supposes either great cultural homogeneity, or major, exceptional interest in this

technique; and all the more so if its assimilation involves difficulties. The mobility of the different culture traits is thus very variable. The qualitative aspect of the invention as well as the qualitative aspect of the environment in which it is transmitted must necessarily be taken into consideration.

(2) Besides the cultural homogeneity of the transmitting and receiving peoples, it is indispensable to consider the social homogeneity of the receiving milieu. When a relative degree of social homogeneity subsists in the latter, transmission is made easily from close to close, from person to person, on a continuous line. However, when social differentiation has been accentuated to the point where it results in a difference in living standard and in a difference in culture between the leader and his entourage on the one hand, and the mass of the population on the other, or between definitely characterized social classes, it follows that political and cultural centers are constituted which impose their decisions and set the pace for vaster and vaster wholes. Transmission then runs the risk of being made, not from person to immediately neighboring person (or even from specialist to specialist), but from a more evolved area to a politico-cultural center some distance from the border dividing the two areas. A secondary diffusion then radiates from the center to the limits of the less evolved area without always reaching them. In France, for example, in the case of techniques which were diffused, apparently, in a Neolithic fashion (harvest flail, vine and wine techniques), one observes that they often diffused from center to center by more or less extended leaps. Recently, there still subsisted on the borders of culture areas, in their contact zone, archaisms of technique as well as of customs and of belief (Parain 1957). It is permissible to think that the processes of cultural differentiation between the tribes and the groups of tribes, as well as the processes of social differentiation within the tribes, had begun to develop as soon as the Neolithic epoch. At least, these processes can not be neglected when it comes to more recent diffusions in the Neolithic fashion.

(3) What is said about the "Neolithic revolution" is relatively secondary with respect to the main subject, but it is worth-while considering because the discussion is on the same ground of the qualitative as opposed to a quantitative interpretation. Even if a large number of traits composing what is usually called the Neolithic revolution originated outside of Iraq and converged there for geographical reasons, this is no reason to deny the appearance of a real revolution in technique in Iraq (or in a

neighboring zone). A revolution is not defined by a juxtaposition, a quantitative accumulation of traits. Only when qualitative transformations are manifested can it really be perceived. Some time before the "revolution," properly speaking, a number of the traits which will enter into its composition are seen to appear here and there, but not one of these traits is revolutionary in itself. The revolution is a process of combining, of the structuring of these elements by the action of a particularly dynamic factor which gives them a shape, a meaning, and an efficacy that are really new (in this case, the cultivation of cereals and other plants, and the domestication of animals).

Far from tending to contest the interest of Edmonson's research and the importance of the results he has already obtained, it seems to me that the preceding reflections should help to overcome the difficulties which will surely arise as soon as he broadens the field of his observations. Data that would be judged to be anomalous or even unlikely, if only the quantitative aspect of the processes is considered, should become elucidated and explained by their qualitative aspects.

By JEAN PERROT*

Edmonson's paper makes stimulating reading, and his hypothesis is tempting; but taking into account the immense difficulties, of which he is perfectly aware, his argument fails to convince. Without discussing the intrinsic value of his hypothesis, these general remarks may be made concerning evidence from the Near East:

(1) With or without the help of C-14 dating, general opinion dates the simultaneous appearance of pottery in the Near East from Iran to Palestine, through Kurdistan, Cilicia and Syria, somewhere in the 6th millennium B.C. The appearance of pottery in this region is not closely linked to a definite level of food production, nor even to sedentary living; nevertheless, it seems to constitute a "rational" trait in the general cultural development which follows the emergence of a food production system based on the cultivation of cereals. The diffusion of pottery is subsequent to the diffusion of cereals.

(2) Pottery makes its appearance in Egypt later than in Southwest Asia, as does the cultivation of cereals. A certain period of time was necessary for wheat and barley—the natural habitat of which was the hilly zone from Palestine to the Zagros—to reach the Nile Valley by difficult roads. In prehistoric times, the Sinai peninsula was an insurmountable barrier and the only roads were the sea

routes. This presupposes the sailing of boats from the Syrian coast to the Delta region and up the Nile to Upper Egypt, or from the Arabian coast through the Red Sea to the African coast and to Egypt through Wadi Hamamat. To anybody familiar with those intermediate regions, the idea of the diffusion of pottery from the Caspian shores to Kenya through the center of the Arabian peninsula sounds fantastic.

(3) The diffusion of copper is quite a different matter, as its first use is independent of the general cultural development. It is certainly in part related to the proximity and the nature of ore deposits. A distinction should be drawn between the diffusion of metal artifacts by way of commercial relations and the propagation of the techniques of metallurgy. Before discussing the reliability of a few C-14 datings it would therefore be necessary to try to determine the degree of technological knowledge attested to by the metal artifacts found on the various sites. There is a far greater difference between the hammering and the casting of copper than between pure copper and bronze. For the specialists in ancient metallurgy, the admixture of tin, probably accidental at first, represents only a minor development. Casting around a core, a complicated process still in use today, was current practice for the metallurgists of Southern Palestine around 3500 B.C., hundreds of years before the appearance of bronze; and still earlier, it was probably known to the semi-nomad population around the Syro-Arabian desert. On the other hand, pre-dynastic Egypt knew only hammered copper, and it is only in the early dynastic period that Egyptians began the exploitation of copper ore in the Sinai peninsula.

(4) C-14 dates in the Near East cannot be considered independently of the general archaeological evidence. As for the hard facts of cultural reality, it is difficult to see at present how they can be better explained than by the concept of cultural complexes.

By MERRICK POSNANSKY*

Edmonson's approach is original, if perhaps somewhat hypothetical. Its tremendous value, which cannot be underestimated, lies in its well considered collation of the data bearing on the diffusion of the "cultural traits" of pottery and copper.

Though the diffusion routes and rates as expressed in the maps are generally convincing, the distinction between the diffusion of pottery and that of pottery manufacture should be realized. The results may have some relevance regarding the spread of the food pro-

duction, with which pottery is normally associated, though the use of the term "Neolithic" is perhaps questionable in the context of the first appearance in an area of pottery and copper.

Though Edmonson has considered the effect of the surface irregularities of the earth's crust on diffusion rates, climatic irregularities are of equal, if not greater, importance. In Africa, the effect of the Saharan desert, the Sudd swamps, the Congo Forest and the less visible barriers of endemic insect-borne disease cannot be overstressed, and must surely affect the diffusion rates as compared with the Northern European Plain and Steppe, for example. A constant arrived at from two such contrasting zones must, to some extent, be suspect. A further complicating factor affecting diffusion rates and their constant must surely lie in the rapid social and economic changes which are consequent in such "origin" centres as the Middle East, and which result in the diffusion of successive traits, many of which may overtake previously dispersed traits. An example of this is the probable simultaneous arrival of pottery and ironworking in the area west of the Great Lakes in Africa. In such a case the rate would depend, not on a Neolithic society as in Central Europe and Asia, but on a more complex Iron Age Society.

What Edmonson's approach does bring out is the general acceptability of radiocarbon dates. His world-wide approach is refreshing, as is his suggestion of a pottery origin in the Ulan Bator vicinity. Hitherto, perhaps too much attention has been focused on Middle Eastern origins.

By IRVING ROUSE*

Edmonson's paper leads me to raise a question of theory. Edmonson makes no reference to the concept of society, whether expressed in the form of a single community, or as the grouping of communities into cultures, phases, or foci (to use alternative terminologies current in the archaeological literature). Throughout his paper, Edmonson refers to the diffusion of traits from person to person, rather than from community to community or from culture to culture.

In my own thinking, "diffusion of traits from person to person" means the process whereby an innovator creates a new custom, or adopts it from a foreign source, and gradually persuades the rest of the people in his community to follow his example. Nonarchaeologists are perhaps unaware of the extent to which archaeologists have investigated this process. It is the basis for our technique of seriation, whereby we determine the age of sites by investigating the increase or decrease in the popularity of traits through time, i.e., the extent to which

the individuals in a community have adopted a new trait or abandoned it in favor of a still newer trait. The theory behind this technique has been well discussed by Ford (1951) among others.

The diffusion of traits from community to community or from culture to culture is another matter. Here, I think it is necessary to make a distinction between (1) diffusion from one community to another within the same culture; and (2) spread from a community in one culture to a community in another culture. Theoretically, (1) should be easier than (2) and should happen more rapidly, since there is no problem of adjustment to a different cultural background. So far as (1) is concerned, therefore, I am inclined to agree with Edmonson that functional interrelationships among traits need not be taken into consideration. I do not agree that this is justifiable in the case of (2), however. Too many instances can be cited in which a group has rejected a new trait, even when it is eminently "rational," because it does not fit their culture.

Osborne (1957) has recently discussed this point with respect to the North Pacific Coast and Plateau areas of North America. He cites several scattered occurrences of pottery in those areas, in some cases fired, and in others not. Apparently, the trait was accepted by some "persons" in these areas (contrary to Figure 2) and even became integrated into several cultures adjacent to the Plains; but the majority of persons and cultures rejected it. If Edmonson's date of 1900 B.C. for the first appearance of pottery on the Plains is correct, and if knowledge of pottery reached the Pacific Northwest soon thereafter, as seems likely, people in the latter area must have rejected it during a period of over 3000 years, except in the few instances enumerated by Osborne.

An example of a somewhat different kind is provided by the diffusion of pottery to the lower Orinoco River. Edmonson considers the date of 900 B.C. for the first appearance of pottery in this area to be relatively reliable. I would agree, but would point out that pottery does not seem to have spread to all of the cultures on the lower Orinoco. There is reason to believe that some of the previous nonceramic cultures survived alongside the pottery-making immigrants, as happened elsewhere in the Caribbean area (Cruxent and Rouse, 1958-59: 14-16). Indeed, one of the nonceramic cultures apparently persisted into the nineteenth century among the Warrau Indians of the Orinoco Delta (personal communication from Johannes Wilbert). It is probable that the ancestors of the Warrau lived alongside people of the

Saladero and subsequent ceramic cultures for nearly 3000 years and that they resisted the diffusion of pottery during that entire time.

By failing to take into consideration instances like these, in which cultures have persisted in rejecting traits accepted by adjacent or coexistent cultures, Edmonson gives a distorted picture of the nature of diffusion. He views it as a "simple," two-dimensional process, taking place in time and "person-space." In my opinion, the process is complicated by a third dimension, that of a culture or society, and Edmonson's model should be revised to include this dimension.

In its present form, the model does not satisfactorily explain why some cultures have adopted traits more rapidly and more completely than others. Why, for example, did the Chinese develop a civilization well before the time of Christ, when our ancestors in Western Europe failed to do so although they had as much, if not more, contact with the center of civilization in the Near East, lived in a similar environment, and occupied a similar topographical position relative to the land masses of the Old World (to use Edmonson's term)? Why, during the 150 years of Eskimo-Norse contact in Greenland, did the Eskimo adopt so many Norse traits and the Norse so few Eskimo traits, especially since the Eskimo traits were better adapted to the environment? Why have the Japanese made better use of the "rational" aspects of Western civilization than their neighbors, the Chinese? Why is the technical assistance which the United States supplies to underdeveloped countries more successful in some cases than in others? I do not believe that these questions can be answered without taking into consideration the concept of a culture.

By J. M. CRUXENT*

Edmonson proposes a world-wide expansion of Neolithic culture by a kind of "railroad line" diffusion from station to station, which seems to me impossible to establish reliably in the light of our present knowledge. There are two phenomena opposing such a large-scale diffusion theory: (1) convergence or parallelism; and (2) unplanned maritime movement. World-wide similarities can never be explained merely by wishing to demonstrate the fact, or by basing one's opinion on a single phenomenon. Cultures with their own distinctive characteristics have arisen in diverse parts of the world, and have diffused through marginal contacts (or osmosis), commerce, conquest, migration, and unplanned "extraordinary voyages" (i.e., by accident).

Ethnology furnishes many examples

of groups that have refused to accept pottery, one of the basic neolithic traits. Rouse, in his comment on this article, cites the Warrau Indians, who, despite some acculturation and evolution, seem to be Meso-Indian survivors. Perhaps they lack pottery today because of environmental limitations, for they live in pile dwellings in the Orinoco River delta, and only with great difficulty could they find suitable clays in this habitat. Possibly in earlier times they inhabited a region which afforded clay, but their way of life did not permit them to accept pottery; if so, perhaps this prejudice allowed them to settle without inconvenience in the Orinoco delta. The difficulty of getting clay there would not have been considered a drawback, since they felt no need for pottery.

Another Venezuelan group, the Chiricoa, who hunt and gather on the plains (*llanos*) of Apure province, confirm the possibility of such an appraisal. Although they live in a region affording excellent raw materials for pottery, and have always been surrounded by ceramic using groups (such as the Achaguas, the Yaruno, the Salivas, and others), the Chiricoa have never made nor used pottery. They reject it not simply because their nomadism makes its transport impractical, and because they lack time to manufacture replacements for broken vessels, but also because when they find vessels made by other groups they dare not use these because of their fear of witchcraft. Beliefs such as this can considerably retard the acceptance of techniques or artifacts by a group. During the time I lived with the Chiricoa, I found them to be very little acculturated. They had accepted only some cloth, which they had stolen, and various Western glass bottles, which their neighbors lack. These bottles were highly prized because of their properties of "impermeability" and "interior visibility," and because they were free of the "neighbor witches" traps.

One can not deny the occurrence of diffusion, but its limitations in passing through time and space must be recognized. Although his study is impressionistic, André Leroi-Gourhan (1946) demonstrates that passage via the Bering Strait was not so easy, rapid, and inevitable as many believe. While accepting the fact of passage through the Strait, I do not think it satisfactorily accounts for the entire population of the Americas. (Even so, the spread of ceramics by neolithic peoples with a rather high technical development could be more easily explained by diffusion than could the astonishing similarities recorded between the Siberian and African lithic, and that of South America—similarities

which at present confront us with problems of great import and greater incomprehensibility.)

It is premature to establish a diffusion route leading from a remote point in the Old World all the way to Patagonia. Convergence and accidental voyages cannot be ignored. Barriers against diffusion have existed for thousands of years to block its path. We have many examples of present-day aborigines who are quite reluctant to accept Western culture, and who reject that of their neighbors. Moreover, we can see how much time it often takes for modern advances to penetrate into rural areas, even though a major city or the capital lies only a few kilometers away. Traditions and superstitions are barriers that are successful over time.

When we closely examine problems of the spread of many culture traits over thousands of kilometers, we think of the thousands and thousands of years that would be necessary for ceramics to reach South America by means of the "railroad route" via the Bering Strait. If this theory were valid, it seems to me that the ceramics of the nuclear centers of diffusion—i.e., the oldest ceramics of the Old World—must be assigned an age of more than 15,000 or 20,000 years. In no other way could one explain their presence by diffusion in the Americas as early as the date of $4,450 \pm 20$ B.P. which Edmonson cites (see Rubin and Alexander 1960: 181).

Chronology everywhere is still in its infancy today. Radiocarbon dates are too few, and one still awaits chronological results in the study of many South American cultures.

It is interesting to note a previous comment on the subject. The problem of world-wide Neolithic similarities was dealt with by A. Vayson de Pradenne who arrived at the following conclusion:

To sum up:—the idea which emerges from the facts set out above is that, instead of regarding the polished-stone cultures of contemporary savages and our own neolithic ancestors as comparable—as the convergent outcome of separate lines of evolution, proceeding at unequal rates—one should regard them both as the outcome of a great shock that shook humanity and roughly levelled up the various industrial stages, being succeeded by a phase of isolated advances moving at unequal rates (1935: 310).

By H. D. SANKALIA*

This is indeed a very novel treatment of the diffusion concept. Regarding the approach itself, I wonder how Edmonson would explain certain cultural "islands" or "backwaters" suggesting a Neolithic or Pastoral stage. I have in

mind the Todas of the Nilgiris, who in spite of the flourishing, highly advanced Iron Age civilization around them persist in retaining a pastoral culture. There must be similar "islands" in many parts of the world, and these would prevent our statistically counting the rate of diffusion. A number of factors are involved in diffusion, and I do not know if these can be simplified in the manner attempted by Edmonson. However, I congratulate the author on his novel approach.

By DOUGLAS W. SCHWARTZ*

It would be interesting to experiment in the New Guinea area, or some region with roughly comparable Neolithic conditions, to see how diffusion rates today compare with those figured archaeologically. The traits chosen would preferably be those not influenced by contact with missionaries or ethnographers, but introduced by the field worker who would later check their progress from group to group.

It is noteworthy that migration does not show up as a more disturbing influence on the diffusion rate as calculated by this method. Although there are a number of examples of well-documented migrations during the prehistoric period (e.g., Thompson 1960), the movement of great numbers of people over a major land area during a relatively short period of time probably did not play an important part in the culture history of the Neolithic period. The role of migration has probably been exaggerated due to the dramatic part this process played in the period after A.D. 1500. Therefore, I would not expect migration frequently to upset any constancy of the diffusion rate proposed.

In Edmonson's discussion of the part the individual plays in this transmission of traits, there is an overtone of cautious determinism. It is true that a remarkable consistency of movement showed up for the traits tested. Perhaps for these traits, individual acceptance or rejection played no important part in speed of transference. But this startling discovery should not overshadow the fact that many traits will never be passed on at all; many are lost at the start, because they are not accepted by some few individuals.

By A. PETROVSKÝ ŠICHMAN*

There is much validity in Edmonson's tendency to put in one line various aspects of the spread of Neolithic civilization. This is shown most strikingly for the Americas. But why was pottery much earlier in Kenya than in Egypt, which is closer to Mongolia?

And what is the effect of water diffusion? There are unpublished hints that sea-farers were already familiar with inland Europe by the first appearance of European pottery.

On the other hand, there are other facts indicating the correctness of Edmonson's theory. For instance, in the Old Egyptian language one agricultural term is etymologically identical with the Indo-European word represented by English WHEAT, and Gothic HWAITEIS; this term must have come to Egypt from Europe or Asia, perhaps simultaneously with the first pottery. A similar cultural flow probably brought to the Near East the influence of the Satem group of Indo-European languages. The oldest sections of the Old Testament were orally transmitted in some language of the Satem group. Further, Y. Ziya's book, *Arier und Turanier*, includes material indicating that the early origins of civilization were in Middle Asia.

By WALTER W. TAYLOR*

Edmonson's model is most suggestive, and the notable consistency of its results indicates that it is meaningful and representative of cultural reality. But precisely what is that meaning, and what cultural reality does it represent? I find myself in a theoretical quandary. On the one hand, the consistency leads me to believe that he has discovered a truth of some moment. On the other hand, it puzzles me how consistency can eventuate and truth can be represented when there are so many significant sources of potential error, and when certain premises which he uses to reduce the possible range of error are, at least to me, so dubious. For example, even given the ideal conditions of his model, I find it difficult to believe that "any single invention . . . would diffuse from its point of origin at a *constant rate* . . . throughout cultural space" (italics mine) or that "by using linear distance along the most plausible route of diffusion . . . we can eliminate [the effect of geographic irregularity] and reduce it to a predictable order." Several alternatives come to mind: that the actual errors are not serious, and/or cancel themselves out; that Edmonson's mitigating premises are indeed valid, and I am wrong in doubting them; or that, by some coincidence, errors and faulty premises have led to valid results, which in reality are based on combinations of factors comparable to, but not identical with, those he has used.

In the present state of our knowledge, I can think of no single, infallible test which can help us choose between these (and other) alternatives. However, I do believe that there may be several ways of producing testimony of a sort. I be-

lieve that archaeology and its increasing number of co-operating disciplines are on the threshold of providing data which should resolve at least some of these difficulties.

For one thing, despite Edmonson's pessimism concerning the available information and his warning as to the limitations of his method, I believe that significant data might be obtained if we used his method to study diffusion in certain areas over shorter spans of time and space. It is obvious that Edmonson's "constant rate" pertains to diffusion on the grand scale. It is a general, mean figure derived from diffusion chains made up of many related, but culturally autonomous, links. Actually, his constant rate is a mathematical parameter derived from contiguous, continuous, but essentially individual, cultural actions. Except in the most extended sense, it does not derive from a single and unitary cultural act. It may be interesting, in a general way and in the "culturological" sense (as Leslie White would say), to know that copper and pottery and the chicken spread over the globe at a uniform rate. But possibly it is more significant to know how and why that uniformity came about, when the global diffusion chains were made up of such diverse and individually contingent links.

Perhaps study in certain archaeologically favored areas might help us understand these links as component parts of the larger diffusion chains, and thus make the latter more culturally meaningful. Perhaps these studies can be made in the Southwest U.S.A. or even in the Greater Southwest, if current work by DiPeso, Kelley, Meighan, and others produces the results we hope it will. I suggest that the Danubian Neolithic, or Bronze and Iron Age Greece, might provide the required data and chronological controls. The Viru Valley in Peru, and diffusion from Roman and Anglo-Saxon Britain into the west and north of the British Isles might be investigated. A prime necessity, of course, is an absolute chronology, or a very solid relative one based upon cultural traits other than those being studied. While not exactly what Edmonson was doing, a profitable study might also be made of the dissemination in time and space of such specific traits as Jeddito Polychrome and St. Johns Polychrome throughout the Greater Southwest, or of faience beads in the Mediterranean-North European area. Crawford's work on the Eye Goddess might serve if, as in the above examples, the method could be made applicable to the diffusion of objects as well as of ideas.

In ending these brief comments, I should like to call attention to one idea of Edmonson's which particularly in-

trigued and stimulated me. I refer to his proposition that "the conjunction of these [Neolithic] traits in the Near East is less a reflection of intrinsic functional or evolutionary interrelations among them than of the geographical position of the area in which the conjunction first occurred. Rather than starting in Iraq and diffusing outward, the 'Neolithic' appears in considerable part to have converged on it." With this centripetal concept (in place of the more usual centrifugal one), I believe that important studies could be made in such areas as the American Southwest, Mexico, Peru, and in fact, any area which contained what the archaeologists have been calling an "area co-tradition."

Finally, let me say that not the least exciting thing about Edmonson's article is the fact that it was written by a person whose previous anthropological interests seem to have been in quite different fields. As an indication that there are still anthropologists with catholic interests and broad competencies in this era of increasing specialization, this is tremendously encouraging. It is pleasing to be reassured that the wide compass which made anthropology great is still with us.

By DOMINIK JOSEF WÖLFEL*

I can not believe that there ever was something like "a homogeneous field of persons," or that inventions travelled "in all directions . . . at a uniform rate of speed." The evidence known to me indicates that new cultural elements or inventions often travelled rapidly to distant regions, whereas areas quite close to their place of origin remained unreceptive. Two thousand years ago, a cultural map of the world would have shown areas of high civilization isolated among very primitive types of culture, their distribution depending on such factors as geographic and climatological conditions, ocean currents and prevailing winds, and the affinity of regions to civilization.

I am convinced that in the sciences of man, or the history of human culture, we are still far from achieving final conclusions or solutions. Of the immense mass of facts necessary for reconstruction, one-half have been irretrievably lost, while only a small part of the other half has been discovered and studied. We should proceed like beginners, trying to form an inventory of the phenomenology of human culture in all its spheres. There are special types of culture that have not yet been recognized as such—for instance the megalithic culture, which in my understanding was the channel leading from the Neolithic cultures to the early high civilizations.

What I appreciated in this article

was the inclusion of high civilizations in Edmonson's argument, as well as the daring spirit of the author.

Reply

By MUNRO S. EDMONSON

What if I am wrong? I have tried to foresee possible sources of error, and my colleagues have added more. I am grateful for their corrections of fact; and, as a nonspecialist in many areas where they are experts, I of course accept their amendments. I can only regret that, having lacked access to an adequate research library or even to my own notes since January, 1960, I can not comment upon them in detail. However, I do not feel able to accept as fact the "general opinion" that Perrot cites for Near Eastern pottery, "with or without the help of C-14 dating": I prefer radiocarbon dates. (I regret having given Perrot the impression that I postulated a diffusion of pottery directly across the Arabian desert; my actual measurements went around its north end and down the Hejaz.)

Personally, I feel that Heine-Geldern's objection that I have grossly underestimated the role of migration in diffusion is the most telling point made by my critics, and would be the most likely explanation for massive error. However, neither Heine-Geldern nor anyone else advances evidence for the diffusion of specific rational Neolithic traits at rates very different from those I have calculated. The fact that migrations may have been very rapid in the Neolithic does not prove that one of them intervened in the diffusion of pottery. Becker's remark that "the Danubian cultures must have spread very rapidly . . ." is intriguing; I should like to know how rapidly.

I am glad to learn (from Heine-Geldern and Obayashi) that my assumption that copper preceded bronze everywhere is incorrect for the Orient. Such data might make possible a much better calculation than mine, possibly giving the use of copper a point of origin closer to the Near East, as Menghin suggests, while locating the origin of bronze rather nearer the Orient. I certainly see no reason to trace the use of either metal from one highly dubious radiocarbon date for the Old Copper culture of North America, as Eggan (who seems to misread my proposition as some sort of wicked pan-diffusionism) suggests I should. (I gather that Griffin's repetition of this suggestion is intended facetiously.) None of the commentators has seriously questioned any of the ten dates for copper I classed as reliable; it is unfortunate that all but one of them were obtained from sites west of Meshed.

My reconstruction for the diffusion of pottery seems to hold up rather better under criticism. I classified twenty-five pottery dates as reliable. About eight of these have been questioned—six by Griffin. (The others are the Panamanian data, which now appear to be superseded, as mentioned in the paper; and the Belt Cave data, which Menghin informs me has recently been appraised as too early.) On the other hand, dates that I think may possibly be uncertain are more highly regarded by some experts. The mean standard error of radiocarbon dates in this time period is about 300 years; it is clear that the mean standard error of professional archaeological opinion is considerably larger. There has been no "sleight-of-hand" in my sticking with (and reporting *all* of) the radiocarbon dates. I do not understand why Griffin, having questioned many of the American dates, did not also question those from Tennessee and Tamaulipas, which are material to the argument. As it is, he seems unique among my critics in his flexibility about radiocarbon error. In challenging all except two of my North American dates, he appears to feel that the reliability of radiocarbon determinations is not much better than 33 per cent. I do not believe I am alone in failing to share his pessimism.

A few colleagues have apprised me of new radiocarbon dates I had not included. I believe that in every case these have appeared only in recent months. Since these dates are only alluded to but not cited, I have no idea how they will fit the construal I have proposed; but I reject the implication that they constitute evidence of data I have wilfully suppressed. Indeed, many of the doubtful dates I have been criticized for including were used precisely to avoid giving this impression, and I thought I had made it clear that my confidence in them was minimal. My critics have, in general, been far more cavalier than I in rejecting radiocarbon dates and juggling estimates—a privilege, of course, of their greater familiarity with the total array of data in their areas. I am nonetheless led to wonder whether some of these opinions do not reflect regional orthodoxies of interpretation rather than hard fact.

Many of these revisions of my dating in fact cancel each other out. I am left with the impression that the facts may eventually support a separate origin for some part of New World ceramics (as I commented in my paper), although this possibility still seems to me somewhat less likely than the construal I have offered. I agree that a closer typological analysis of the instances of early pottery

Edmonson : NEOLITHIC DIFFUSION RATES

would be most rewarding, and would doubtless invalidate some of my interpretations and judgments. I remain skeptical of its disproving my hypothesis. It seems to me striking that disagreements among those who mistrust my reconstruction (and between them and me) are notably larger when the radiocarbon method is not used than when we can agree to adhere to this source of absolute dates. To me, this seems to suggest the presence of markedly subjective elements in such judgments, and accordingly I feel confirmed in my original decision to stay as closely as possible to radiocarbon data, whose errors, while potentially massive, are at least free of doctrinaire bias—mine, or that of anyone else.

Nothing constrains me to argue that, because diffusion can proceed at a rate of better than a mile per year, it therefore must. I am aware that even rational traits may encounter quite irrational resistance, as in the Venezuelan examples cited by Cruxent and Rouse, or the Indic ones mentioned by San-kalia. I believe that such phenomena are subject to explanation, even though not under the present rubric. My concern here has been with how fast rational diffusion under Neolithic conditions *can* be, not how fast it *must* be. It is quite conceivable to me that a strategically located tribe—barring the Bering Strait route, for instance—could hold up the diffusion of pottery to a large area—like the Americas—almost indefinitely, through some unpredictable taboo. But I do not see anything particularly American or anti-historical in saying that this does not appear to have occurred. In fact, some such circumstance may have been responsible for the marked time-lag in the diffusion of bronze to sub-Saharan Africa.

I believe that my colleagues' most serious factual doubts concern the dates for Jomon pottery, and the diffusion of bronze and copper in the Orient. Errors here could well invalidate my specific reconstructions; but they might do so without necessarily disproving my general thesis. Part of my procedure may not have been clearly explained. The fact is that the three oldest radiocarbon dates presently available for pottery in the Old World, and a rate of 1.15 miles per year (calculated from six traits, of which pottery was only one), indicated a point of origin in Mongolia from which the same rate and map distances come within 200 or so years of several of the earliest radiocarbon dated pottery sites in North America. I do not understand what Eggan means by *predict* or *account* for; but that is what I meant. In preparing my final calculations for

pottery, I adjusted the point of origin to fit the American and Old World data slightly better, but this was not necessary. I did not wish to lean too heavily on the incredibly early date cited for Jomon pottery. Griffin's new Honshu date, however, appears to make this caution somewhat less necessary. I am not a partisan of any particular origin for ceramics, or for any other trait; and, in fact, the rate of diffusion is not dependent upon the place of origin of the trait in question. It is quite possible that pottery diffused at a rate of about 1.15 miles per year whether it was invented once, or several times.

If it is true, as Eggan asserts dogmatically, that the New World data for copper and bronze "clearly contradict" my hypothesis, I should like to see the evidence. What little I have been able to learn about the matter is neither so clear nor so contradictory as he indicates. These data have been omitted from the discussion, not, as Eggan says, because they were contradictory, but because I was unable to get enough "clear" data for a plausible reconstruction of any kind. Archaeological colleagues inform me that my very provisional linkage of the Old Copper culture to the Arizona Hohokam is untenable. (I may note in passing that my routing assumption here was that diffusion occurred by way of Mexico, as the distance of 3,400 miles suggests, but which I had not stated.) I therefore await the testimony of those who are more expert than I in New World metallurgy; but I decline to be cowed by pronouncements unaccompanied by data.

Sichman's question about the appearance of pottery in Kenya much earlier than in Egypt seems answered by the information sent me by Posnansky, indicating that it very likely was not earlier. In fact, it seems to me that my critics are often in far more extreme disagreement with one another than with me. While I am somewhat taken aback by the volume of commentary, I see in its diversity the normal reaction of independent minds to a somewhat novel idea that is judged by the varying standards of differing experience. Becker's remark that I am half a century behind the times is balanced by other opinions that my hypothesis is original (Posnansky), new (Parain), novel (Sankalia), or even daring (Wölfel).

I am sorry that I have "failed to convince" nine of my colleagues and downright outraged one or two, but I am not persuaded that what I have argued is, as Becker concludes, "impossible." I plead guilty to having committed errors that a specialist in particular areas would not have made; but on balance I do not believe that any of these errors, nor all

of them together, disprove my hypothesis.

And what if I am right? I was, frankly, rather surprised that eight colleagues were already prepared to give some measure of credence to my hypothesis; the hypothesis itself led me to expect rather more cultural resistance to even such a simple and rational idea as a constant diffusion rate. The obvious misgivings of my more favorable critics are therefore heartening, rather than the reverse. It is striking that even those most favorably inclined do not follow me all the way; that is, no one appears to be sufficiently convinced of the relative invariance of these rates to confront the problem of why they prove so stable, or even to question my explanation. I am tempted to think that no one really believes it—yet. I need hardly add that I do not consider that indefinitely prolonged rejection of my hypothesis will prove it right; but I have some confidence that the idea may eventually find some measure of acceptance. Otherwise, I shall regretfully conclude that it is less rational than I had thought.

Perhaps the most recurrent theoretical criticism is that directed at my remarks about trait complexes and evolution; Fernández-Méndez' points about the latter seem to me particularly discriminating. If the cultural resistance to the diffusion of simple rational inventions is indeed subject to relatively little variation measured in the number of sequential persons reached per unit of time, then this fact would necessarily be reflected in very complex ways in the immensely tangled web of real history. Given traits could become associated, temporally and spatially, in arrangements that were not particularly logical, giving rise to assemblages in which, for example, pottery occurs in one region with an otherwise Mesolithic assortment of artifacts, while in another area it is invariably associated with Neolithic inventions. I see little gain in describing the relationship between pottery and the other traits in these two cases by the term "complex." And if "complex" implies some necessarily functional relationship, its application to traits known to have diffused separately is obviously erroneous. If, as I believe, such inventions as pottery and the use of copper or bronze diffused separately, rather than as part of Neolithic, Chalcolithic, or Bronze Age complexes, then their inclusion in such complexes is not simply a matter of taste; it is a mistake.

I have in no sense rejected "the concept of culture complexes," but only its application to a certain class of cultural traits—simple rational inventions; and I have repeatedly noted that the diffusional dynamics of "complex systems or patterns" must be quite differ-

ent. I believe this distinction can be empirically made. A definition of the Neolithic in terms of the conjunction of a limited number of simple rational inventions—polished stone, pottery, cultivated plants, domestic animals—may be taxonomically useful, but it cannot bind together in a "complex" traits known to have been assorted independently. It is not I who have dealt badly with this conception of the Neolithic; it is history.

I agree with Griffin's stricture that "Neolithic conditions" is somewhat confusing shorthand for the circumstances of communication I have tried to examine; and particularly in the light of the above-mentioned problems of an adequate definition of "Neolithic." But no other expression of comparable brevity comes to mind. I think I have made it clear that this usage is only an analogy, and that it refers to any type of cultural communication in any period of history that is relatively unaffected by specialization of knowledge and division of labor. I believe we may theoretically expect changes in these circumstances to affect the geographic diffusion rate; this seems to have happened with the relatively late spread of the flying gallop and the diffusion of tea. I doubt that such effects are to be anticipated in the Archaic Period of North America, for which Griffin particularly disputes my use of "Neolithic conditions." I am prepared to believe they were present in the "Bronze Age," although our data do not seem to reflect them as yet.

The widespread slow diffusion of technological ideas seems incompatible with any phrasing of the process of cultural evolution that is known to me. This is not to deny the cumulative effects of such ideas, nor their frequently implicative relationships to each other. But I believe that, in any particular case, this evolutionary or quasi-evolutionary effect can be explained by the wealth or poverty of primary inventions that diffusion makes available to people at particular points in the communicative network of cultural history—Neolithic or modern—coupled with the conception of the relatively random occurrence of creative innovation—of secondary or derivative inventions (the "qualitative transformations" to which Parain refers). Cultural complexity, a state of relative "evolutionary" advancement, is thus to be expected at those points enjoying the best communications with the rest of mankind (past and present), whether defined geographically, as seems fairly feasible for Neolithic times, or in terms of social organization, writing, libraries, and geography-defeating, time-conquering communications, as would be necessary in our own period. This construal, I be-

lieve, offers a way of differentiating those inventions that actually depend upon previous knowledge from those which merely happen to follow in a given order in some particular case, by virtue of geographic or communicative accidents. I neither like nor dislike the word "evolution"; and if neo-evolutionary theorists see it as applicable to this process, that is all right with me. The implication of species autonomy inherent in the biological analogy seems to me quite misleading in reference to a cultural process, some of whose genes (simple rational inventions) appear to circulate with only minimal respect for species (cultural) boundaries.

From the standpoint of accounting for the high civilizations, I find Rouse's assertion of the similarity between the locations of China and Europe totally baffling. The mere disparity in size between Asia and Europe seems to negate such a statement, and a plausible interpretation of routes of communication must surely place the two areas very differently. I still believe that a careful study of this problem would show that under Neolithic conditions the Ordos was more accessible to large human populations than was Europe, and that its earlier advancement can be adequately explained on this basis.

I have every respect for the climatic and ecological problems raised by Kroeber, Posnansky, Wölfel, and others. But it seems possible that if the relevant communication can be entirely verbal (which is conceivable for simple rational techniques), rather than requiring bulk trade or prolonged and intimate contact, even very tenuous routes of diffusional communication may be adequate for the transmission of simple traits. I am, therefore, inclined to think that some natural obstacles—mountains, forests, disease zones—may have a greater effect on the transmission of complicated traits than on simple traits.

I am forced to take issue with Menghin's position that "environmental circumstances . . . cannot be neglected in long-range calculations." I believe they can. On the other hand, I am convinced that they may be decisively important in shorter-range considerations. This problem seems susceptible to empirical resolution. As we control better data, this source of diffusional variation should be reflected in irregularities in the apparent diffusion rates. So far, the data seems to suggest that such effects are quite local, and, at least in most cases, as short-range as two or three hundred miles or years.

In conclusion, I do not believe that I have proved that simple rational diffusion takes place at a constant rate. I do believe that I have made a good *prima facie* case for a relatively re-

stricted range of variation—sufficiently restricted to require explanation. I see no overriding theoretical objections to this belief; and I await with some confidence and some skepticism the proofs suggested by Schwartz and Taylor or alluded to by some of my doubting colleagues that, under genuinely Neolithic conditions, a simple rational trait actually did diffuse at a rate of a different order of magnitude than that of 1.15 miles per year.

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Several Problems in the Study of Kinship Systems

by D. A. Olderogge

PROBLEMS connected with the study of kinship systems have claimed the attention of ethnographers only since the appearance of L. H. Morgan's study, *Systems of Consanguinity and Affinity of the Human Family*. This fundamental work, which gathers together a wealth of material concerning different kinship systems, served to open a new field of ethnography.

It is true that, long before Morgan, many ethnographers studying the customs and languages of peoples of different countries had pointed out the distinctive terminology of kinship. An ancient seventeenth-century grammar, for instance, includes a description of the Bakongo kinship system, which appears to be the earliest account of the kinship system of any Bantu group, and yields a great deal of material from which one may draw conclusions about the development of this system over a period of three hundred years. It is likely that a thorough study of linguistic and ethnographic materials would reveal other records of kinship systems that were compiled long before Morgan's time. But there is no doubt that Morgan holds the distinction of having introduced a new object of study into the realm of science.

The year 1957 marked the eightieth anniversary of the publication of Morgan's *Ancient Society*, which portrays the evolution of the family and kinship on the basis of a study of kinship systems. This book, which was highly regarded by Marx and Engels, marked a turning point in the history of ethnography. After its publication, problems connected with the study of kinship systems came to be the subject of animated discussion. The literature concerning these problems is very extensive.

One unquestionable service that Morgan rendered the study of kinship systems was his pointing out their importance as a first-rate source of historical material. Indeed, the very fact that the kinship system of the

Iroquois—the so-called Dakota type—coincides with those of the Dravidian peoples of South India points to the presence of law-governed processes, through which these systems, whose terminology is absolutely distinct, came to have identical structures. Obviously the social conditions that gave rise to them were identical.

The remarkable uniformity of several types of kinship systems found in different parts of the world made it possible for Morgan to use kinship systems as a basis for reconstructing certain aspects of the evolution of human society, and on the basis of these aspects, reconstructing past stages in the history of the family. Morgan's basic idea was the recognition of the close relation between kinship systems and the structure of society. Today we know that not all of his propositions are indisputable; but this particular idea that a given type of kinship system is connected with the social structure, is valid.

Among the different types of kinship systems are variants known today in ethnographic literature as systems of the "Omaha" type. This system, which was first noted by Morgan, and was later described in detail by Dorsey for the Omaha Indian tribe, is found in other parts of the world as well. The Omaha Indians lived under a patriarchy, in patrilocal settlements, and reckoned clanship in the father's line. Their kinship system was marked by certain distinctive features, particularly the designation of the mother's brother and all of his descendants by special terms. Correspondingly, the line of the father's sister and all her descendants is also set apart, and is considered to be "junior" as compared with the mother's brother's line. It is impossible here to examine all the details of this terminology; but it is noteworthy that the same features are found in the kinship systems, not only of various tribes in North America, but also among the Araucanians of South America; among many tribes and peoples of Southeast Asia (the Angami-Naga, the Ao Naga, and others); in East Africa (among the Bari, Lango, Nandi, and others); among a group of Shona tribes, etc. In all these cases, the tribes and peoples that use this terminology are today, or have been in the past, patriarchal, patrilineal, and patrilocal. Without doubt, we have here a law-governed, not a chance, phenomenon.

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We may be convinced of this by examining another type of kinship system: the "Crow." This system, first described among the Crow Indians, is found among several other North American tribes: the Choctaw, Creek, Cherokee, etc. The kinship systems of these tribes exhibit certain unique features that are directly opposite to those of the Omaha-type systems. The entire line of the mother's brother and his descendants is considered to be junior with respect to the line of the father's sister. Corresponding to this, both lines are distinguished by special terms, and by certain features of kinship nomenclature that are characteristic of this system alone. In the past, all tribes having a kinship system of the Crow type have been matrilinear and matrilocal. As new data have accumulated, this type of kinship system has also been found in South America and West Africa; and in all cases, only among tribes and peoples that have a matriarchy, with its inherent features of matrilineality and matrilocality.¹ These examples show that kinship systems actually reflect definite forms of social organization, and are not accidental.

The eighty years that have passed since the publication of Morgan's work have been filled with bitter conflict over his basic propositions, particularly with respect to the significance of kinship systems. In this polemic, certain trends have appeared in sharp relief. Attempting to disprove Morgan's basic propositions, many American and English ethnographers tried to prove that kinship systems are not connected with social structure, do not reflect actual social relationships, and thus can not be used as a foundation for reconstructing the forms of development of the family and of society.

Best known in this connection are the views of A. L. Kroeber, who denied any logic whatsoever in the terminology of kinship. According to his views, it reflects only different categories of relationship, which as a whole do not constitute any system whatsoever. Kinship terminology reflects psychological characteristics alone, and is determined by language. It is impossible to draw any sociological conclusions from an analysis of kinship terms, and in comparing different systems, one can determine only which of them express one category of relationships or another.²

In contrast to Kroeber, Robert Lowie does not deny that kinship systems reflect social relationships; but, according to his views, it is impossible to reconstruct the history of the development of society. It is thus perfectly natural that all of Lowie's conclusions are specific: he notes that kinship systems have been influenced by various elements of the social structure and various institutions (such as the levirate and sororate, for example), which conditioned the symmetry of such kinship systems as the Dakota type to one degree or another.

It is neither possible nor necessary here to touch upon

the views of other scholars who have denied the possibility of reconstructing the evolution of society on the basis of kinship systems. These include, among others, Andrew Lang, with his theory of the heraldic origin of kinship nomenclature; Kunov, who saw in kinship terminology indices of the generations; and Thomas, the student of Australia, who saw in it, not the expression of marriage relationships and consanguinity, but an indication of the social status of the individual. Thus they took exception to Morgan's basic idea—which both Marx and Engels considered to be of great worth—that kinship systems reflect the form of development of society—or more precisely, of family and marriage relationships characteristic of a given society.

In the first quarter of the twentieth century, criticism of Morgan's propositions was facilitated by the fact that Morgan's adherents, in defending his theory, went off on the wrong path. In trying to explain all of the difficult cases through the interpretation of kinship terminology, they began to devise anomalous types of marriages, supposedly existing in the given societies. For instance, the terminology of certain kinship systems designates members of different generations, such as grandfather and grandson, or grandmother and granddaughter, by the same term; and individuals separated by two or three generations are free to address each other as husband and wife, or even to refer to each other by these terms. All of these features are vestiges of a system of marriage classes, and, in my opinion, of a system of circular connection of kin, having as its basis the Giliak phratry.³ They may be explained in this way, but only by rather complex means. But adherents of Morgan's theory, following the methods of the German lawyer, Josef Kohler, and the English ethnologists W. H. R. Rivers and Brenda Seligmann, interpreted these features too literally, and postulated gerontocracy, in which grandfathers and great-grandfathers always married their granddaughters and great-granddaughters, while grandmothers and great-grandmothers married their grandsons and great-grandsons. A number of such preposterous and sometimes monstrous types of marriage were invented, and it is not surprising that this method of defending Morgan's propositions only served to discredit his views and made it easier for his opponents to criticize him.⁴

Soviet investigators who studied the problems arising in the study of kinship systems and the analysis of kinship terminology took as their point of departure Morgan's fundamental proposition of the connection between classificatory kinship systems and the social organization of the primitive community. Holding that a kinship system reflects the norms of family and marriage relationships in a given society, they regard the

¹ There is a rather extensive literature about kinship systems of the Omaha and Crow types. The list of examples given by Murdock (See G. P. Murdock, *Social Structure*, New York, 1949) may be supplemented.

² A. L. Kroeber, "Classificatory Systems of Relationship," *Journal of the Royal Anthropological Institute*, Vol. XXIX, 1909, pp. 77-84. For an exposition of Kroeber's views, and additional material on them, see A. N. Maksimov, "Sistemy rodstva (Kinship systems)," *Etnograficheskoe obozrenie* (Ethnographic Review), 1909, No. 4:79-83.

³ See D. A. Olderogge, "Koltsevaya svyaz rodov, ili trekhrodovoi soyuz" (Circular connection of kin, or gens triplex), *Kratkie soobshcheniya In-ta etnografii* (Brief reports of the Ethnographic Institute), No. 1, 1946; by the same author, "Trekhrodovoi soyuz v Yuku-Vostochnoi Azii" (Gens triplex in Southeast Asia), *Sovetskaiia Etnografiia*, 1946, No. 4.

⁴ See J. Kohler, "Zur Geschichte der Ehe, Totemismus, Gruppenrecht, Mutterrecht," Stuttgart, 1897 (*Zeitschrift für vergleichende Rechtswissenschaft*, Vol. XII, and different editions); W. H. Rivers, *Kinship and social organisation*, London, 1914, and *The history of Melanesian society* (2 vols.), Cambridge, 1914; Brenda Seligmann, "The marital gerontocracy in Africa," *Journal of the Royal Anthropological Institute*, Vol. 192.

kinship system as one source of historical material. This has been expressed very clearly by S. A. Tokarev in his study dealing with the Australian kinship systems:⁵

In controversies over this question [of the primitive forms of family and marriage relationships], the so-called classificatory systems of designating kinship, which were first described scientifically and used in this relationship by Morgan, usually play an important role. For this reason, the controversy to this day proceeds under the slogans 'for' or 'against' Morgan.

Speaking of the logical error committed by many students of kinship systems (both adherents and opponents of Morgan's theories), Tokarev makes the fine point that while they affirm the group-meaning of kinship terms, they explain the origin of this as a result of the logical extension of the individual meaning of the terms.

Another investigator, E. Yu. Krichevskii, who also studied Australian kinship systems, came to the conclusion that they were based on "forms of dual marriage and class organization with its characteristic cross-cousin group marriage."⁶ Without going into a detailed examination of the arguments advanced by Krichevskii and Tokarev, I shall note only that both investigators regarded kinship systems as reflections of actual social relationships.

However, in defending Morgan's propositions and developing them in the light of Marxist-Leninist teachings, Soviet ethnographers have had to take into account all of the new data accumulated by scientists during the eighty years since the appearance of Morgan's book.

At the time that Morgan was collecting his data, during the 1860's and 1870's, the ethnography and history of the peoples of Oceania were virtually unstudied. From reports of travelers who had visited the islands of Oceania, it was known only that the Polynesians were not familiar with the use of iron, did not know the bow and arrow, and did not have domestic animals. On these grounds, Morgan assumed that the Polynesians belonged to the category of the most primitive peoples, and consequently that their kinship systems must reflect the most ancient condition of society. Today, we know that the social structure of the Hawaiians was incomparably more complex than Morgan had assumed.⁷ Study of the material culture of the peoples of Oceania has shown their amazing achievements in the realm of navigation: these "Vikings of the Pacific," in their sailing vessels, reached the Arctic boundaries of the Pacific Ocean; they were perfectly familiar with wind and current direction; and the construction of

their outrigger vessels can in no way be regarded as primitive. In New Zealand, deep excavation beneath peat deposits has uncovered a bow, and thus established the use of this weapon by Polynesians in earlier times.⁸ Thus Morgan's assumption that the Polynesians were a primitive people has proved to be erroneous. In this connection, proceeding from the basic propositions of Marxist-Leninist teaching concerning the development of social production and forms of society, it has been necessary to re-examine Morgan's assumption of the primitiveness of the Hawaiians and of the Malayan-type kinship systems prevailing among them.

Engels, speaking of the earliest stages in the evolution of forms of the family, felt he could point out, on the basis of new data, that Morgan went too far in his assumptions concerning the earliest stage of the forms of the family and marriage. Engels held the study of the various forms of Australian marriage classes to be very important, considering them (together with the Hawaiian family) to be one of the sources on the origin of kinship. Today, we may say with assurance that Engels was entirely correct, and that the Australian marriage classes constitute that form from which subsequently there developed the different institutions of tribal society.

Long ago, individual investigators—Rivers, Frazer, Thurnwald, and Sternberg—noted that the Malayan features in kinship systems arise as a result of the simplification of the Turanian-Ganowanian systems. However, the general conclusion that the Malayan system was secondary was not drawn from these separate observations. Among Soviet investigators, the first in essence to raise this question was A. M. Zolotarev.⁹ Subsequent investigations have made it clear that the Malayan kinship system must be recognized as secondary:¹⁰ it arises as the result of disintegration of the tribal organization of primitive society. Kinship systems of the Turanian-Ganowanian type are forerunners of systems of the Malayan type. This may be traced in examples drawn from the ethnography of Oceania as well as Africa.

Today, therefore, we consider that the order of development of kinship systems was the following: First, we have the Australian marriage classes. Turanian-Ganowanian kinship systems are characteristic of the tribal structure, and are of several types, depending on its form. Upon the disintegration of the tribal community, the complex system of kinship terminology, which distinguishes between the paternal and maternal kinship lines and among different categories of relatives, becomes unnecessary and disappears. Becoming simplified, the terminology assumes the forms of the Malayan system, examples of which are found among many peoples. Gradually, with the disappearance of different vestigial forms of the tribal society and the disintegration of extended patriarchal families, there

⁵ S. A. Tokarev, "O sistemakh rodstva u avstraliitsev" (On the kinship systems of the Australians) (K voprosu o proiskhozhdenii semi) (Concerning the question of the origin of the family), *Etnografiia*, 1929, No. 1, P. 24.

⁶ E. Yu. Krichevskii, "Sistemy rodstva kak istochnik rekonstruktsii razvitiya sotsialnoi organizatsii avstraliiskikh plemen" (Kinship systems as source material for the reconstruction of the development of the social organization of the Australian tribes), *Voprosy istorii doklassovogo obshchestva* (Problems in the history of pre-class society), collection of articles on the fiftieth anniversary of the appearance of the book of Fr. Engels, *Proiskhozhdenie semi, chastnoi sobstvennosti i gosudarstva* (Origin of the family, private property, and government), Moscow-Leningrad, 1936, p. 327.

⁷ See D. D. Tumarkin, "K voprosu o formakh semi u gavaitsev v kontse XVIII—nachale XIX v." (On the problem of the forms of the family among the Hawaiians at the end of the eighteenth and the beginning of the nineteenth century), *Sov. Etnografiia*, 1954, No. 4.

⁸ See E. Best, "The Maori," Wellington, 1924 (*Memoirs of the Polynesian Society*, Vol. V).

⁹ A. M. Zolotarev, "K istorii rannikh form gruppovogo braka" (On the history of early forms of group marriage), *Uch. zap. Istoricheskogo fakulteta Moskovskogo oblastnogo pedagogicheskogo in-ta*, (Transactions of the history faculty of the pedagogical institute of Moscow oblast), Vol. II, Moscow, 1940, pp. 144-169.

emerge the systems that Morgan calls "descriptive."

The term "descriptive" is unfortunate, since the kinship systems of European peoples in some respects are more "classificatory" in character than those of the Turanian-Ganowanian type, which are called classificatory. It is sufficient to say that the terms designating uncles and aunts in European kinship systems generalize a group of relatives who are differentiated in the "classificatory" systems of the Turanian-Ganowanian type. On the other hand, many purely descriptive terms are often found in the kinship terminology of this latter type. In general, we may say that "classificatory" systems should not be regarded as being opposed to "descriptive" systems, since both systems include terms of both types.

The crux of the matter lies in the fact that kinship systems of a tribal society classify relatives differently from systems of contemporary European peoples, which might be called systems that reflect family relationships in class societies. For this reason, one should probably speak of different types of kinship systems of a tribal society, and of kinship systems of a class society, where tribal relationships have disappeared and kinship relationships are determined on the basis of the individual's relation to the group composed of his nearest relatives, within the limits of the narrow circle of the family. Here we have in mind, not the so-called extended family, but the small family of contemporary society.

Apparently the initial form of social relationships, from which the subsequent kinship systems of the tribal society developed, were systems of marriage classes. Traces of this are preserved in the kinship terminology of many peoples who have classificatory systems. An analysis of the system of marriage classes show that at the basis of many, if not of all, of these were the norms of the "Giliak phratry."

I believe that after the works of Morgan in the field of kinship systems, no work has been more important than L. Ya. Sternberg's study of the Giliak kinship system.¹¹ The Giliak phratry discovered by Sternberg is really the "ideal form of the Punaluan family," and constitutes tribal group marriage where the husbands are brothers, and the wives sisters. But according to Morgan's definition, the Punaluan family constituted the group-marriage of several sisters with husbands who were not necessarily kinsmen of each other, or the group-marriage of several brothers with wives who were not necessarily relatives. This definition is manifestly erroneous, because it would permit the presence of some groups of men or women who were not related to each other.¹²

¹⁰ See D. A. Olderogge, "Malaiskaya sistema rodstva" (The Malayan kinship system), sb. "Rodovoe obshchestvo" (The tribal society), *Trudy In-ta etnografii AN SSSR* (Transactions of the Ethnographic Institute of the Academy of Sciences of the USSR), Vol. XIV, Moscow, 1951, pp. 28-66.

¹¹ L. Ya. Sternberg, "Gilyaki" (The Giliaks), Moscow, 1905 (Reprint from the journal *Etnograficheskoe obozrenie*, Books 60, 61, and 63). See also his work, "Sotsialnaya organizatsiya gilyakov" (The social organization of the Giliaks) in the book: L. Ya. Sternberg, *Gilyaki, Orochi, Goldy, Negidaltzy, Ainy, Stati i materialy*, Khabarovsk, 1933, pp. 129-184, or in the book: L. Ya. Sternberg, *Semya i rod u narodov Severo-Vostochnoi Azii* (The family and tribe among the peoples of Northeastern Asia), Leningrad, 1933, pp. 60-144.

Sternberg's discovery showed that the Punaluan family results in the Turanian kinship system.

The norms of the Giliak phratry existed, not only among the Giliaks, but also among many peoples and tribes of Assam and Burma, among the Batak of Sumatra, and among some of the tribes of northern and north-western Australia. These same norms are found also among many tribes and peoples of Africa. The Giliak phratry (or the norms of the gens triplex) is connected with so-called asymmetrical kinship systems. During recent years, many studies have appeared on this subject.¹³ At the Twenty-Ninth Congress of Americanists, for example, the noted French scholar Claude Lévi-Strauss, in his report dealing with the social structure of central and eastern Brazil, came to the conclusion that underlying dual organization, or to be more precise, as he himself writes, "underlying dualism and the apparent symmetry of social structure, one may divine a more basic three-way asymmetrical organization."¹⁴

It is impossible to dwell here on an examination of this very special question. It is sufficient to note that Lévi-Strauss also came to the conclusion that the gens triplex organization existed in the past; of this I wrote as long ago as 1944. There is no doubt that the course of my argument is different from that of Lévi-Strauss; but it is noteworthy that the problems of the Giliak phratry are only now beginning to receive the importance they merit in the elaboration of the evolution of kinship systems.

Thus, it becomes clear that Engels was right when he asserted that the starting point of the institution of the tribe "might have been the Australian class system."¹⁵

The analysis of the system of Australian marriage classes is very important. This analysis demands precision of terminology and considerable patience. The student of Australian kinship and marriage class systems has at his disposal a great deal of excellent recorded data; and the accuracy of this material is readily verified by details included in the records, which have often been mistaken for errors. Actually, these apparent inaccuracies are results of strict conformity to law, and the problem lies in understanding the essence of the system.

¹² L. Ya. Sternberg, "Turano-Ganovanskaya sistema i narody Severo-Vostochnoi Azii" (The Turanian-Ganowanian system and the peoples of Northeastern Asia) in the book *Semya i rod u narodov Severo-Vostochnoi Azii*, p. 149.

¹³ The works mentioned above by the author of this article, "Koltsevaya svyaz rodov ili trekhrodovoi soyuz" (The circular connection of tribes or gens triplex) and "Trekhrodovoi soyuz v Yugo-Vostochnoi Azii" (Gens triplex in Southeast Asia) deal with these problems, and also "Iz istorii semi i braka" (From the history of the family and of marriage), *Sov. Etnografiya*, 1947, No. 1.

¹⁴ The problems of the connection of clans among the tribes of Burma and Assam are discussed in the study: B. Ruhemann, "The Relationship terms of some Hill Tribes of Burma and Assam," *Southwestern Journal of Anthropology*, 1948, Vol. 4, No. 2. At present these problems are being studied by R. Needham. See: R. Needham, "Circulating connubium," *Bijdragen tot de Land-, Taal-, en Volkenkunde*, 1957; see also R. F. Salisbury, "Asymmetrical marriage systems," *American Anthropologist*, 1956, pp. 639-55.

¹⁵ Cf. Lévi-Strauss, "Les structures sociales dans le Brésil Central et Oriental," in *Indian Tribes of Aboriginal America*, Selected Papers of the XXIX International Congress of Americanists, Vol. 3, Chicago, 1952, p. 310.

¹⁶ Fr. Engels, *Proiskhozhdenie semi, chastnoi sobstvennosti i gosudarstva* (The origin of the family, private property, and government), Gospolitizdat, 1953, p. 40.

An analysis of the marriage class systems of Australia and Melanesia is essential in facing the problem of the origin of classificatory kinship systems of the Turanian-Ganowanian type—or, to be more exact, different types of kinship systems of the tribal society. A number of scholars have engaged in this study, but many of them have confused the problem in considerable measure.

Thus, the theory of a gerontocracy, devised by Rivers and developed in the works of his students and followers (B. Seligmann and others), has proved to be completely erroneous. In order to explain the kinship system of Raga Island, Rivers devised completely aberrant forms of marriage, such as the marriage of grandfathers with granddaughters, and the marriage of men with the wives of their sister's sons or their mother's brothers, etc. Actually, all of these cases can be explained much more simply. Yu. M. Likhtenberg, in her study of the kinship system of Raga Island, showed that at the basis of these matrimonial norms lie the principles of the Giliak phratry.¹⁶ In her subsequent work, Likhtenberg analyzed the matrimonial norms and kinship systems of the Australian tribes, and showed that the kinship terminology reflects various ancient norms, preserved in certain terms as vestiges. So-called reciprocal terms found in the kinship terminology reflect former marriage groups or classes, whose number does not remain constant. Among the Murngin tribe, for example, the former eight-class system has been replaced by a seven-class system. On Ambrym, the eight-class system became a six-class system as a result of the disappearance of two classes. In the face of these changes, the survival of earlier kinship terms based on the presence of eight classes has naturally led to apparent anomalies.

On the basis of careful analysis of the Australian kinship systems, Likhtenberg came to the conclusion that these systems are the result of a long historical development. Apparently, formerly matrilineal and matrilocal groups have modified their composition, and have turned, first, into patrilineal groups, and then into patri-

lineal groups. As a result, the entire kinship terminology and the designation of matrimonial classes became complicated.

An inevitable result of the transformation of earlier tribal relationships was also the appearance of individual kinship terms as well as group terms. It might be said in passing that in order to analyze kinship systems correctly, it is essential to distinguish between these two categories of terms. And it is precisely in this respect, as was pointed out above, that a great many errors have been committed by scholars in the past (among them Morgan himself), who have not been sufficiently precise in discriminating between individual and group terms.

In this brief article, I cannot dwell in detail on the work of all the ethnographers who have studied kinship systems during the past forty years, and I have limited myself to an examination of the basic problems connected with kinship systems. However, it would be a dereliction not to note that today, in connection with the extensive scope of scientific expeditions, there have been collected many data on the kinship systems of a number of peoples of the Soviet Union. It is sufficient to point to the work of N. P. Dyrenkova, L. E. Karunovskaya, L. P. Potapov, S. M. Abramzon, T. A. Zhdanko, K. L. Zadykhina, N. A. Koslyakov, N. K. Karger, N. A. Lipskaya, A. F. Anisimov, A. A. Popov, G. M. Vasilevich, E. D. Prokofeva, and many others. Unfortunately, the majority of works which include records of kinship systems are scattered among various local publications, often difficult to obtain.

There is no doubt that many kinship systems of the peoples of Central Asia have features which resemble those of systems of the Omaha type, a fact that to date has received no attention and has not been noted in the literature. It would be of great value if the Ethnographic Institute were to undertake a publication of the records of the kinship systems of the peoples of the Soviet Union, particularly of the Turkic-speaking peoples, whose kinship systems as yet have not been the object of comparative study from either the linguistic point of view or the standpoint of their ethnographic significance.

¹⁶ Yu. M. Likhtenberg, "Sistema rodstva ostrova Raga i vopros o gerontokratii v Melanesii" (The kinship system of Raga Island and the question of gerontocracy in Melanesia), *Sbornik Muzeia antropologii i etnografii*, (Collected articles of the Museum of anthropology and ethnography), XII, 1949, pp. 254-76.

ARE AFRICAN CULTIVATORS TO BE CALLED "PEASANTS"?

by L. A. Fallers

THE TERM "peasant" has often been used of African rural folk, particularly when distinguishing them from political and religious elites in the larger and more complex societies. The word for the ordinary peasant cultivator in the kingdom of Buganda—*mukopi*—is commonly translated as "peasant," for example, and the literature of French Africa is full of references to "*paysans*." But most writers, one suspects, have used the word rather loosely; if pressed, most of us would be inclined to say: "Well, perhaps they are not *quite* peasants." Africanists tend to feel, perhaps, that the common folk of the complex African polities fall between the categories commonly utilized by those who have studied "peasant societies" in Latin America, Europe and Asia, on the one hand, and, on the other, those employed by students of aboriginal North American "tribes." They strike us as being not quite peasant, but not quite tribal—something in between, a *tertium quid*. We may try to understand more clearly just what this *tertium quid* quality consists in by comparing some African societies with the notion of the "peasant society" developed during the past twenty years by students of Latin American, Asian and European peoples. The point of this is not, of course, merely to play with definitions but rather to explore some of the implications of the fact—of which we are all aware—that the concept "peasant society" refers to a bundle of features which do not always go together. In doing this we may hope to indicate somewhat more precisely how these complex African societies resemble or differ from the classical peasant societies—thus satisfying our anthropologist's urge to fit all the peoples of the world into a grand classificatory scheme—and to suggest, indirectly, something about the significance of those peasant-like features which Africans do not share.

We may appropriately begin with Kroeber's definition (or description), since it has seemed to Robert Redfield, George Foster, Eric Wolf, and other peasant specialists to best describe their field of interest: "Peasants are definitely rural—yet live in relation to market

towns; they form a class segment of a larger population which usually contains urban centers, sometimes metropolitan capitals. They constitute part-societies with part-cultures" (Kroeber 1948: 284). This last phrase—"part-societies with part-cultures"—we may take to be the heart of the matter. It can, however, be made somewhat more precise. On the one hand, the phrase *does* quite satisfactorily differentiate peasant societies from the societies we usually describe as "tribal"—societies whose constituent units, or segments, to use Durkheim's phrase, are all much alike and internally more or less homogeneous, in both structure and culture. Peasant societies, as Kroeber's definition suggests, are more differentiated, both socially and culturally. Peasant villages may all be much alike, but they are bound together into a larger whole by structures of a quite different kind, and the persons who man these other structures commonly have a different culture—a "sophisticated" or "urban" or "elite" or "high" culture. On the other hand, Kroeber's definition does not satisfactorily distinguish peasant societies from modern industrial ones, for the constituent units of these latter are also "part-societies with part-cultures"—even more partial than are those of peasant societies, because modern societies are even more differentiated. We may suggest that the single most important difference between peasant and modern industrial societies lies in the nature of their constituent units. Whereas in peasant societies the household and the local community remain the primary units, in modern industrial societies occupational structures dissociated from households and cutting across local community units become important centers of cultural and structural differentiation. It is the vast increase in the differentiation and autonomy of occupational structures which industrialization makes possible above all. Local communities thus lose their semi-self-sufficiency, the semi-detached quality which they still retain in the peasant society and which led Redfield to emphasize—indeed to over-emphasize in his earlier work, as Foster has pointed

out—the similarity between the peasant village and the tribal segment (Redfield 1947; Foster 1953). Unlike the latter, the peasant community is not completely isolable, completely capable of self-sufficiency; but neither is it so completely knitted into a larger fabric by criss-crossing occupational structures as is the modern community. The latter cannot possibly be imagined in isolation from the larger society. In contrast, the peasant community is relatively self-sufficient, leading many observers to comment upon its frequent indifference to changes in the political superstructure and hostility to members of the elite. Perhaps we may usefully alter Kroeber's characterization to read as follows: A peasant society is one whose primary constituent units are semi-autonomous local communities with semi-autonomous cultures. In this way we may differentiate the peasant society from both the tribal and the modern industrial varieties. It is perhaps necessary to add that we think of these as ideal, not concrete, types; actual societies will be in varying degrees "tribal," "peasant," and "modern industrial." The types are a means toward greater understanding, not a device for pigeon-holing whole societies.

Now this semi-autonomy of constituent local communities, which we may take to be the differentiating characteristic of the peasant society, may be decomposed into a number of aspects, of which we may here consider three: (1) the economic, (2) the political, and (3) what we may call, perhaps not very satisfactorily, the "cultural." We shall examine each of these briefly, attempting to see how far they find counterparts in the more complex African societies. It should perhaps be repeated that we consider here only trans-Saharan pagan Africa, excluding the Muslim areas and Ethiopia (see map).

1. In economic terms, a peasant is presumably a man who produces—usually through cultivation—mainly for his own household's consumption, but who also produces something to exchange in a market for other goods and services. This is the economic aspect of the peasant community's semi-autonomy (Firth 1951: 87). In this sense, peasants abound in Africa. The vast majority of Africans were cultivators in pre-contact times—in some cases intensive and devoted cultivators. They also traded; the great markets of West Africa are famous, but also in most other parts of the continent there was a good deal of craft specialization and trade aboriginally. Cowrie shells, gold dust, ivory, iron bars and hoes in different areas provided semi-generalized media of exchange. In the more centralized states there was tribute, and sometimes even regular taxa-

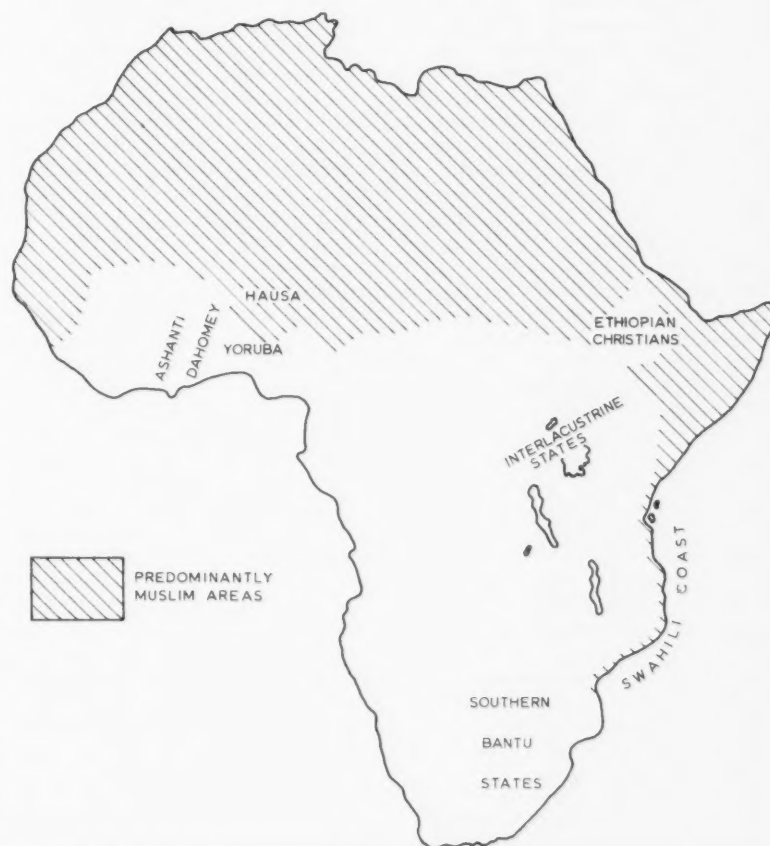
tion, to support and augment the authority of the non-agricultural superstructure. In short, in the economic aspect of the matter there appears to be no problem. Economically, most Africans were traditionally peasants and with the opening of the continent to overseas trade they quite easily and naturally took up the cultivation of export crops in exchange for imported goods.

2. Politically, too, the more powerful African states had much in common with the Asian, European and American societies which are commonly classed as "peasant." The political aspect of the peasant society has received relatively little attention from anthropologists, perhaps because so often the societies which the peasant specialists study have long since been "decapitated" politically (to use Kidder's expressive phrase), the indigenous political superstructure being replaced by those of modern colonial or nationalist post-colonial states (Redfield 1956: 78). The political aspect is thus the one least accessible to study through the anthropologist's traditional techniques of direct observation. We can learn more about this aspect of the society made up of semi-autonomous peasant local communities from social historians like Marc Bloch, from legal scholars like Maine, Maitland, Seebohm, and Vinogradoff and from comparative sociologists like Max Weber. The legal scholars who unravelled the political structure of medieval Western Europe and its characteristic unit, the manor, divided into two schools, the "Romanists" and the "Germanists," over just this question of the nature of the semi-autonomy of communities which lies at the heart of the peasant type (Vinogradoff 1892: 1-39). The Romanists emphasized the vertical relationship of the manor with the political hierarchy, associating it historically with the Roman *latifundiae* established in the area during the time of the Empire. The Germanists, on the other hand, saw in the manor and its surrounding "vill" essentially a development out of the old solidary Germanic village community. If we understand this literature correctly—and it is easy for an anthropologist to go astray in the writings of legal historians—both were right, in the sense that both tendencies were present. They were debating, in an historical idiom, the relative importance of the two dimensions which always seem to be present in the peasant society and which constitute the political aspect of the semi-autonomy of its constituent local communities: On the one hand there is the local community, hostile to the outside, sharing certain common rights in land and governed by local, often informal, mechanisms of social control; and on the other hand there is the hier-

archy of patrimonial or feudal relations of personal superiority and responsibility (*noblesse oblige*) and subordinate dependence, which link the local community with the wider polity. Peasant political systems vary with the relative strength of the local community as against that of the vertical structure, as Eric Wolf (1955) has pointed out; and also, as Max Weber (1947: 346-58) has shown, with the degree to which the latter is made up of appointed personal retainers as contrasted with hereditary feudal (using the term loosely) vassals.

These political peasant-like features, like the economic ones, are common enough in African states. The village community is a common feature everywhere (though it may be physically dispersed in scattered homesteads) and political hierarchies, where they exist, vary according to the degree to which they consist of appointed officials or hereditary chiefs. Africa is, of course, preeminently the continent of unilineal descent groups and this gives to both village communities and political superstructures a character which is less common in other regions. The village community often contains a core of lineages and its corporate nature may be

expressed in the idiom of unilineal descent (Fortes 1953; Smith 1956). There is an interesting range of variation according to whether strangers in such communities are relegated to a kind of second-class citizenship or are fictionally adopted into the core group. These phenomena are not, of course, universal in Africa. In the Interlacustrine area, the lineage is limited to such "domestic" functions as the control of inheritance and exogamy, and the village itself is defined purely territorially (Fallers 1960). In the political superstructure of African states, unilineal descent groups often hold corporate rights to chieftainship; and thus the hierarchy, which in medieval Europe tended to consist of dynasties of individual hereditary lords and vassals, in an African state like Ashanti, or in one of the Southern Bantu states, consists essentially of representatives of corporate descent groups (Busia 1951; Schapera 1955). In some other states, like Dahomey and Buganda, the hierarchy is essentially one of patrimonial retainers, resembling in this respect the states of the Islamic world and Byzantium more than those of feudal Europe (Herskovits 1938; Fallers 1960). In general, allowing for peculiar-



Map showing Islamic areas of Africa and some of the major pagan states.

ities resulting from differences in patterns of descent, the politics of the traditional African states seem to fall well within the peasant range.

3. Thus, there would seem to be no reason why African villagers should not be called "peasants" politically and economically. Doubts arise, however, when we turn to the culture that is characteristic of peasant life—that is, to the tendency, to which Redfield (1956), Foster (1953), and Marriott (1955), have drawn attention, for the economic and political semi-autonomy of the peasant community to be matched by a cultural semi-autonomy. The culture of the peasant community of the classic conception is a "folk" version of a "high culture"; it is neither the same as the latter nor independent of it, but rather a reinterpretation and reintegration of many elements of the high culture with other elements peculiar to the peasant village. It is this cultural semi-autonomy of the village, it would seem, which above all determines the relations which obtain between social strata in the peasant society. The elite possess, and live by, the high culture to a greater extent than do the peasants. The peasant, accepting the standards of the high culture to some degree, to that degree also accepts its judgment of him as ignorant and uncouth. At the same time, he possesses his own folk culture, containing high culture elements, and this provides him with an independent basis for a sense of self-esteem, together with an ideology within which he may express his partial hostility toward the elite and its version of the common culture. Excellent examples are provided by Homans' account (1940: 368–370) of how thirteenth century English villagers caricatured the behavior of the elite.

Now it would seem to be just the relative absence of this differentiation into high and folk cultures which principally distinguishes the African kingdoms from the societies which have commonly been called "peasant." There is, of course, a substantial degree of cultural differentiation in many African societies. There are craft specialists with highly-developed skills, and there are ritual specialists with great bodies of esoteric knowledge. There are courtly manners and there are recognized degrees of sophistication, ranging from the courtier who is "in the know" politically to the country bumpkin. Nevertheless, there remains an important difference between trans-Saharan pagan Africa in these respects and the differentiation which was possible in medieval Europe, China, India and Islam. The word "peasant" denotes, among other things,

a degree of rusticity in comparison with his betters which we do not feel justified in attributing to the African villager.

Lacking the more pronounced degrees of cultural differentiation, the African states characteristically exhibit a somewhat different pattern of stratification. African villagers do not seem to feel the same degree of ambivalence toward the political superstructure that European, Asian and Latin American peasants do because, not standing to the same degree in contrast to the possessors of a differentiated high culture, they do not to the same extent feel judged from above by a set of standards which they cannot attain. Correspondingly, there is less development of a differentiated folk culture as a kind of "counter-culture." Africans very commonly perceive themselves as being differentiated in terms of wealth and power but they do not often, except in the few real composite conquest states, view their societies as consisting of "layers" of persons with differential possession of a high culture. As is well known, they much more characteristically divide in terms of genealogical and territorial segments, even in instances where there are marked "objective class differences" in the Marxian sense. Not even those West African societies in which cities provided a basis for rural-urban cultural differentiation exhibit the degree of folk-high distinction commonly found in what we may now call the real peasant societies. Dahomean and Yoruba villagers were not separated from their urban cousins by a cultural gap of the same magnitude as that which divided medieval European and Asian countrymen from city folk (Herskovits 1938; Bascom 1951; Lloyd 1955).

In large part this difference is due simply to the absence in traditional Africa of the literary religious traditions which formed the bases for the European and Asian high cultures. Written records make possible a vastly greater accumulation and elaboration of high culture. Furthermore, the mere presence of writing places between the literate and the illiterate member of the same society a barrier which cannot easily develop in its absence. The peasant's suspicious hostility toward the member of the urban elite is in large part a product of his realization that the latter, in his ability to read and write, holds a weapon against which the peasant cannot easily defend himself.

Thus the traditional African villager was, we might say, a peasant economically and politically, but was not a peasant culturally. Perhaps it would be

better to call him a "proto-peasant" or an "incipient peasant," for wherever literary culture has entered Africa, it has quickly made him more fully a peasant. Thus the Muslim Swahili peoples of the east coast and the Hausa of the Western Sudan were traditionally more peasant-like than their non-literate, pagan neighbors; and the modern Baganda and Ashanti, with their imported Western Christian high culture, are more fully peasants than were their great-grandfathers. We may suggest that one of the reasons why Christianity, Islam and their accompanying high cultures have been so readily accepted in many parts of Africa is that many African societies were structurally "ready" to receive peasant cultures.

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THE OBSIDIAN DATING METHOD

by *Donavan L. Clark*

DURING THE past few years, brief statements have occasionally been released concerning the development of a new method for dating archeological materials composed of obsidian (volcanic glass). To date, these comments have been confined to scientific meetings, or to journals with restricted distribution (e.g., I. Friedman and R. L. Smith, *Transactions of the American Geophysical Union*, 1958; Friedman and Smith, *American Antiquity*, 1960; C. Evans and B. Meggers, *ibid.*). Now, through the medium of CURRENT ANTHROPOLOGY, we can report on the method to anthropologists throughout the world, and thereby solicit their co-operation in the continuing research necessary to perfect obsidian dating. In particular, we should like to inform the Associates of CURRENT ANTHROPOLOGY about the nature of obsidian and the process of hydration; and to suggest the principles, as well as some of the limitations, of establishing hydration rates and archeological chronologies.

The "fathers" of this method of dating are two United States Geological Survey scientists, Irving Friedman and Robert L. Smith, who, after several years' research on the properties and "ageing" of obsidian, established the correlation between hydration thickness and time.

A brief review of the natural formation of obsidian, and of the investigations of these geologists, should aid in understanding the method and its attendant problems. In simplest terms, obsidian may be thought of as a volcanic or extrusive rock of "arrested development," a fact of considerable relevance to its subsequent alteration history. In areas of volcanic activity (e.g., the circum-Pacific continental margins, the circum-Mediterranean area, Africa, and Oceania), magma under pressure passes upward through fissures, faults, or other zones of weakness in the earth's crust, and flows to the surface as a molten, viscous lava. The external portions of the flow are quickly chilled or "quenched" upon exposure to the atmosphere. Should this rapid solidification occur before crystallization can proceed, the resulting formation will be vol-

canic glass, a rock which is chemically and physically unstable, and tends to alter through time.

Devitrification (gradual crystallization) is one form of obsidian alteration; but the duration of this process is presumably of the order of millions of years, and hence it is of geological significance only.

The alteration process that concerns us here is *hydration*, or the diffusion of water from the environment (meteoric water) into the surface of the solid obsidian. As a result of its peculiar origin, obsidian is a unique rock. In a geophysical sense, it can even be considered a "frozen liquid," since it is non-crystalline and retains some of its ability to flow. Another special property is its tendency to take up available water—i.e., the process of hydration. All the atmospheres and alluvia where obsidian extrudes, even in the most arid lands, are sufficiently aqueous for hydration to proceed. Non-hydrated obsidian usually contains less than 0.3 per cent by weight of water, a state of chemical disequilibrium. For some sort of "stability" to be reached, obsidian thus takes in water from the surrounding air or surficial (soil) environment. Actually, for reasons that will be explained, a stability is never completely achieved; for in the process of hydrating, obsidian is physically and chemically altered, ultimately becoming another petrological substance called *perlite*. In one sense, *perlite* may be considered a decomposition product of obsidian.

The hydration process can be outlined as follows. First, a covering film of water of molecular thickness is formed (absorbed) on all freshly-exposed or newly fractured or flaked (and hence, non-hydrated) obsidian surfaces. The process of uniform diffusion then begins, so that water drawn from the surface film advances (is absorbed) into the obsidian as a sharply demarcated front. We call this "water-saturated" zone the *layer of hydration*. A hydration layer always contains about ten times more water than the interior, non-hydrated, portions of obsidian, and consequently has a higher index of refraction than the unaffected portions. Similarly, its

greater water content increases the density of the hydration layer and modifies its volume. The discrete difference in properties between a layer of hydration and the non-hydrated portion sets up mechanical strains throughout the layer, which can be recognized when examined under the microscope. The optical phenomenon thus produced is called "strain birefringence." When a thin section is cut from an obsidian artifact, mounted on a microscopic slide, and viewed under polarized light, the layer of hydration will appear as a luminescent band. Under ordinary light, owing to its different index of refraction, the layer will exhibit a distinct phase contrast. Normally, the boundary or interface between the hydrated and non-hydrated portions will stand out sharply, and the depth of hydration may thus be measured under high magnification.

Depending upon the type of obsidian and its history of hydration, a layer will "grow" to a thickness of about one micron (0.001 mm.) after one hundred years of uninterrupted hydration. Rhyolitic obsidian from central California (which has had a temperate, Mediterranean type climate for the past several thousand years), for example, a hydration rate of about one and one-quarter microns in 500 years; two microns in 1000 years; three and one-half microns in 2000 years; and about seven microns in 5000 years. In areas of gross climatic extremes, however, the rates of hydration may differ considerably. Thus, the hydration rate of obsidian from equatorial regions is about forty times faster than that of obsidian collected from permafrost zones of the Arctic.

Layers normally continue "growing" until the zone of hydration reaches a thickness greater than fifty microns, whereupon a critical point of cumulative strain is reached, and the layer spalls off. When this event occurs is admittedly obscure; and spalling depends in part upon fortuitous mechanical agencies. But as far as we can foretell at this writing, that is the only factor that will impose a chronological limit upon the dating potentialities of the method. And if our estimate is correct, the effective range for dating with obsidian will be more than 200,000 years.

Friedman and Smith proposed, on theoretical grounds, that the hydration of obsidian proceeds at a rate which is uniform for a given environment. To demonstrate the fact, they needed specimens for which specific data concerning age, provenience, and conditions of interment and weathering would be available. In other words, the most desirable obsidians would be those whose histories of fracturing or chipping were known to the collector, or could be determined, at

least, from their location and stratigraphy. The obvious source for these would be obsidian artifacts from known archeological cultures, preferably those which had independently been dated by Carbon-14, ceramic analysis, dendrochronology, written records, or other established standards of chronology. Thus, they turned to archeologists for assistance, and obtained specimens from stratigraphic excavations and dated cultures in various parts of the world. However, the archeological interpretations soon became so involved that Friedman and Smith requested the aid of two Smithsonian archeologists, Clifford Evans and Betty J. Meggers. The latter contributed their advice, as well as a large number of obsidian artifacts from deep stratigraphic excavations on the coast of Ecuador. The results of these first years of research on artifact specimens have now been published (Friedman and Smith, Part I; Evans and Meggers, Part II, 1960), with discussions by the geologists of theory and methodology, and an evaluation of the method from a cultural standpoint by the archeologists.

Two years ago, aided by Wenner-Gren funds, I learned the techniques of the method in the laboratories of Friedman and Smith in Washington, D. C., and began a study of obsidian artifacts from central California—a region most favorable for this research, in terms of an abundance of material, as well as control of the natural and cultural variables. From July, 1959, to June, 1960, under the provisions of a National Science Foundation Grant, and under the direction of B. A. Gerow of the Department of Anthropology, Stanford University, I collected and analyzed hydration data on artifacts from nearly all the important sites in central California. Enlisting the aid of interested statisticians and mathematicians at Stanford and the U. S. Geological Survey, Menlo Park, we subjected the method to some rigorous tests for statistical significance. These data also permitted us to ascertain a hydration rate for central California, which is based upon five reasonably close correlations with radiocarbon dates. We were especially fortunate in being supplied with well-documented material by Robert Heizer, Director of the Archaeological Survey, Department of Anthropology, University of California, Berkeley. It is hoped that this research will be published in the near future as a doctoral thesis.

Concurrent with the California project, Friedman, Smith, Evans, and Meggers had worked out rates of hydration for world climatic zones, based upon a variety of material from Asia, Africa, the Middle East, and parts of North and South America. Insofar as the ar-

cheological and correlative information submitted is accurate, it is the consensus of all these workers that in the next phase of research it will be possible to develop expressions of the hydration rate for all the major obsidian areas of the world. Ultimately, we should be able to provide a "universal" formula for dating obsidian objects, which will contain appropriate constants to account for regional variables.

The participants in obsidian research have been aware of the necessity for continuing basic investigations before this dating method became prematurely "accepted" as a routine geochronological procedure. It was decided that the continuing research could best be conducted in Washington, D.C., where close collaboration with Friedman and Smith could be maintained, and technical advice and assistance could be readily obtained from Smithsonian Institution personnel. A two-year research grant was received from the National Science Foundation to conduct this program, under the direction of Clifford Evans, with the author as full-time research assistant. In September, 1960, we began this phase of the work with the

following objectives: (1) to determine the nature of all possible factors which control or modify rates of hydration; (2) to classify obsidian artifacts and obsidian rock from natural sources on a worldwide basis according to differences in composition and texture, with the possibility that these identifications can give evidence of quarry sources, as well as trade routes and diffusion among aboriginal peoples; (3) to establish definitively regional rates of hydration from which absolute dating can be derived; and (4) to standardize the techniques of the method for routine use in the fields of archeology and geochronology.

A consideration of some of the problems inherent in the method suggests that the above objectives constitute a rather large order, but none of these problems is beyond solution, provided we can acquire suitable and sufficient samples. The major problems appear to be those of environmental and compositional variables. Early in their research, Friedman and Smith predicted, on theoretical grounds, that temperature is the main factor controlling the rate of hydration. They subsequently demon-

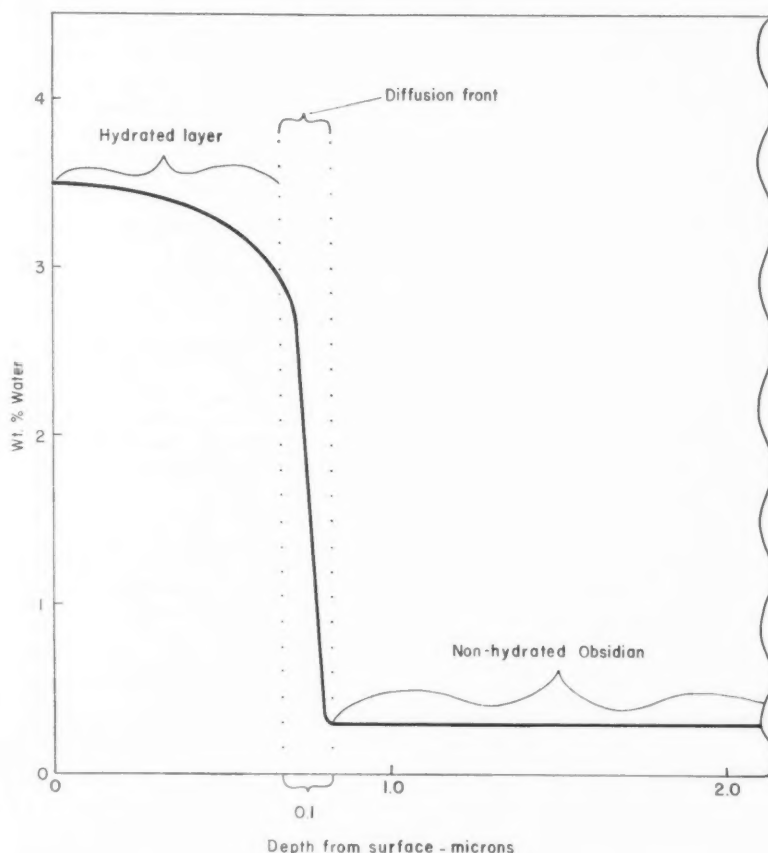


FIG. 1. Contrast in water content of hydrated obsidian edge. Steepness of the diffusion front results in a clearly visible interfacial boundary (Friedman and Smith 1960).

strated, with obsidian artifact specimens, that the rate at which water will diffuse into obsidian is regulated by surficial (soil) temperatures. These climates or seasonal ranges of temperature vary with latitude and altitude; they may also have shifted and modified over Pleistocene and Recent time, compounding the problem (particularly for the interpretation of archeological obsidian from the earlier sites). However, the climatic factor, while posing a formidable problem, is not insurmountable. As soil temperatures and their relationship to air temperatures are recorded, zones of soil climate can be mapped, and hydration measurements can then be correlated in terms of local or regional temperature constants.

Another geological factor which may influence hydration (but to a lesser extent than temperature) is variation in the chemical compositions of obsidian. Like climatic variations, this is a regional problem. Obsidian under a special set of geologic conditions (e.g., that in the rift valleys of Africa) has somewhat different properties than the more typical rhyolitic obsidians. We are now fairly certain that significantly varied types of obsidian may have different rates of hydration. From the practical point of view, this problem should not present a serious block to the development of the method, since for most geo-archeological provinces the obsidians studied thus far appear to be homogeneous. Should composition prove to be a very important variable, we should be able to work out different rates for each major obsidian province. Interestingly, it is possible to discount relative humidity as a significant factor in the hydration process—a point explained in detail by Friedman and Smith in their technical article (1960).

The total problem of perfecting this dating method also requires a better understanding of some of the archeological aspects. Whether an artifact is truly contemporaneous with its associated cultural complex is one of the primary questions to be answered. As every archeologist knows, this is a very difficult question; but much of our quandary can be eliminated by restricting obsidian research to the examination of only those artifacts which possess reliable associational data. We intend, therefore, to solicit from archeologists only those specimens for which they have detailed information about the conditions of collection, associated materials, estimated age, results of dating by radiocarbon or other methods, etc. This sort of information will greatly reduce the degree of uncertainty as to what the obsidian method is attempting to date.

Evans and Meggers (1960) have dis-

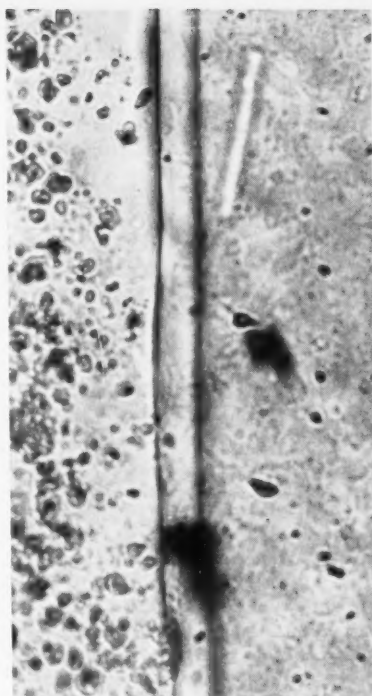


FIG. 2. Artifact from preceramic, possibly Lithic, site in Puebla, Mexico (G334). Archaeological estimate 7000–5000 years B.P. Hydration thickness 8.7 microns. Rod-like object in upper portion of field is a feldspar microlite; smaller dark objects are mostly magnetite. Magnified 450 diameters.

cussed some of the cultural problems that have arisen in obsidian dating thus far, and have rather convincingly shown that obsidian artifacts from earlier cultures had been re-used in certain cultures in Ecuador. Discussions with other archeologists, working in California, Alaska, and Mexico, confirm the belief that re-use of obsidian is an ancient and widespread trait. Indeed, the concept of aboriginal peoples as being themselves "collectors of antiquities" and "possessors of heirlooms" has not been sufficiently considered in the archeological literature. My work with California obsidian artifacts has convinced me that, in addition to the hoarding and re-use of artifacts, older specimens were frequently reworked. In a few instances, for example, I have observed projectile points which were obviously refashioned from larger chipped implements belonging to a much earlier horizon. In such cases, it is not surprising to find that the artifact retains minor portions of thicker, and hence older, hydration layers. Instances of such dual layers may arise as frequently as once in a dozen specimens. We must conclude that the thicker layer of hydration is either the original, natural surface, or



FIG. 3. Artifact from Tanaki Site, Hokkaido, Japan (0522b). Associated with pottery of Satsumon Haji period, older than 1000 years B.P. Hydration layer is 3.1 microns. Gross color of specimen is red; dark objects oblique to the edge of hydration are partially collapsed vesicles, lined with an iron oxide. Magnified 450 diameters.

represents the period of initial manufacture; the thinner layer, then, would represent the later cultural context or, less likely, a post-cultural fracture. In the solution of similar archeological problems, we are fairly sure that careful laboratory analysis can often reconstruct the history of usage of the artifact specimen.

In order to accomplish the objectives outlined above, it is necessary that the project gain the interest and cooperation of archeologists and prehistorians throughout the world. We have suggested that much of the doubt and confusion, to which all new dating methods seem heir, can be dissipated by judicious sampling. Equally important is adequate sampling. These two points will be stressed and repeated. Thus, obsidian specimens sent for testing should in all cases be clearly associated with, and representative of, a distinct cultural context. Ideally, they should be specimens from sites which have been dated by other methods: e.g., radiocarbon, dendrochronology, ceramic sequences, written records, or other typological and stratigraphic criteria. In cases of specimens from C-14 dated sites, the stratigraphic relationship of the obsidian to

the carbon sample level should be clearly specified.

Obsidian artifacts which are typologically distinct are clearly good specimens; although chips and other artifactual by-products are also usable. Chips are especially useful if they can be provided in quantity, since the sample will then have greater statistical reliability than single artifacts. Artifacts collected from the surface or from ambiguous contexts should be considered for study only as a last resort, and the accompanying data should clearly state this information. Layers of hydration on obsidian which has been burned are apparently destroyed for measuring purposes, and this material should not be included if any other obsidian is available. (In-

cidentally, only obsidian and similar types of glass show hydration; such lithic materials as flint, chalcedony, jasper, quartz, quartzite, etc., are not suitable for this method.)

We hope that this general statement, combined with the more detailed article appearing in *American Antiquity* (April, 1960), will be sufficient to gain the co-operation of all the Associates of CURRENT ANTHROPOLOGY. This co-operation will help perfect the method and give it universal utility. Before sending obsidian specimens, please communicate directly with us, describing the sample, the cultures the specimens refer to, their stratigraphic relationships, etc. After this notification, we will send the donor detailed shipping instructions,

forms for submitting data and samples, etc.

Address your correspondence to Dr. Clifford Evans, or to Donavan L. Clark, Division of Archeology, U. S. National Museum, Smithsonian Institution, Washington 25, D. C., U. S. A.

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Research Reports

New Guinea Languages

This report is condensed from a more extended paper, "The Languages of the Eastern, Western, and Southern Highlands Territory of Papua and New Guinea," which is being published in A. Capell, *Linguistic Survey of the South-Western Pacific*, new and revised edition, South Pacific Commission, Noumea. A summary is published here, both for its intrinsic value as new data from an area of growing interest, and to give advance notice of the book in which it will soon appear.

Almost the entire area of the three Highlands Districts of Papua and New Guinea, as well as a few small regions beyond their borders, is occupied by a single group of interrelated languages. This group, which is the largest known group of related languages in New Guinea, constitutes a stock consisting of five families,¹ one of which is represented by a single language only.

Although individual languages of this stock had become known through the work of missionaries, their similarities were first recognized in 1948 by A. Capell. In 1958, S. Wurm made a linguistic survey of all accessible parts of the Highlands Districts, discerned the interrelations and geographical range of the stock, and established the classification given below.

As a result of work by missionaries, officials, and anthropologists, as well as the research of Capell, Wurm, and members of the Summer Institute of Lin-

¹ According to M. Swadesh (Ed., *Amerindian Non-Cultural Vocabularies*, revised edition 1955): Languages sharing between 12% and 28% of their basic vocabularies are said to belong to the same *stock* whereas languages sharing between 28% and 81% are said to belong to the same *family*.

guistics, New Guinea Branch, almost all languages in the three Highlands Districts are now "known," though information about individual languages varies considerably in extent, accuracy, and reliability. Unfortunately, very little of the material available could be made generally accessible through publication. Besides pioneering work on the few remaining unknown or little-known languages, detailed depth-studies and exhaustive lexical investigations of individual languages are needed. Such linguistic problems as the tonal qualities of the languages in the Highlands Districts also call for further detailed work.

The East New Guinea Highlands Stock (approximately 730,000 speakers) shows the composition when proceeding from east to west that is indicated in the Table.

The Gadsup-Auyana-Awa-Tairora Family, the Gende-Siane-Gahuka Kamano-Fore Family, and the Chimbu-Chuave Sub-Family of the Hagen-Wahgi-Jimi-Chimbu Family are almost wholly situated within the borders of the Eastern Highlands District. Benabena overflows into the Morobe District near Mt. Helwig; Chimbu, into the Minj Sub-District of the Western Highlands; while Gende is wholly located in the Madang District around Bundi. Biyom adjoins Gende on the southeast. The remaining sub-families of the Hagen-Wahgi-Jimi-Chimbu Family, the Enga-Ipili Sub-Family of the Enga-Huli-Pole-Wiru Family, the northern fringe of the Huli area, and almost all of the Duna area are in the Western Highlands District. The Wahgi language overlaps into the Eastern Highlands and in the Kup Census Division of the Chimbu Sub-District; and Enga overflows into the Sepik District on the lower Yuat. The remaining sub-families of the Enga-Huli-Pole-Wiru Family are situated in the Southern Highlands District.

Agreement in basic vocabulary between languages belonging to different families within the Stock averages 18-25%, with lower or higher figures appearing in only a few instances. Languages belonging to different sub-families within the same family usually show 40-55% agreement; and languages belonging to the same sub-family usually agree in more than 60% of their basic vocabulary.

In the majority of cases, these varying degrees of agreement in basic vocabulary are matched by comparable agreement in language structure. There are, however, a few languages which constitute connecting links between individual families and sub-families. With such languages, the structural evidence of relationship may present a somewhat confused picture, and in some instances findings on the lexical and structural levels may even tend to be contradictory.

Of the languages which are intermediate between families, Gimi and Wiru may be mentioned as examples. Gimi shares a higher percentage of basic vocabulary with languages of the Gende-Siane-Gahuka-Kamano-Fore Family than with those of the Gadsup-Auyana-Awa-Tairora Family, but is structurally much more similar to the latter. Wiru's aberrant position in the Enga-Huli-Pole-Wiru Family is borne out by lexical and structural evidence. However, it also shows considerable lexical agreement with the Wahgi language, of the Hagen-Wahgi-Jimi-Chimbu Family, and with Mikaru.

Benabena, Yabiyufa and Ipili may be cited as examples of languages which constitute links between sub-families. Benabena is structurally almost identical with Gahuku, of the Gahuku-Benabena Sub-Family, but shows a higher percentage of agreement in basic vocabulary with Kamano, of the

The East New Guinea Highlands Linguistic Stock

FROM EAST TO WEST

	No. Speakers		No. Speakers
GADSUP-AUYANA-AWA-TAIRORA FAMILY	29,979 ¹	HAGEN-WAHGI-JIMI-CHIMBU FAMILY (Continued)	
GADSUP-OYANA SUB-FAMILY	15,227	CHIMBU-CHUAVE SUB-FAMILY	141,791
Gadsup	6,338	<i>Chimbu-Sinasina Division</i>	
Agarabe (1951)	7,958	Chimbu proper (Kuman)	60,273
Oyana	931	Dom dialects	40,949
AUYANA-USURUFA SUB-FAMILY	5,263	Dom proper	14,834
Auyana	4,073	Marigl	16,665
Kosena	341	Salt	7,107
Usurufa	849	South Marigl	2,343
AWA SUB-FAMILY	1,185	Sinasina	15,608
Awa	1,185	<i>Chuave-Nomane Division</i>	
TAIRORA-BINUMARIEN SUB-FAMILY	8,304	Chuave	19,104
Tairora	8,181	Sua	4,290
Binumarien	123	Nomane	1,567
GENDE-SIANE-GAHUKU-KAMANO-FORE FAMILY ..	143,959	ENGA-HULI-POLE-WIRU FAMILY	229,831
+ 8,400 estimated		+ over 22,000 estimated	
GENDE-BIYOM SUB-FAMILY	approximately 8,400	ENGA-IPILI SUB-FAMILY	100,533
Gende	app. 8,000	+ over 22,000 estimated	
Biyom	app. 400	<i>Enga Division</i>	
SIANE SUB-FAMILY	19,800	Kyaka (Baiyer)	9,000
Siane dialects	15,336	Enga dialects	91,533
Koreipa	2,467	+ over 17,500 estimated	
Lambau	5,521	Wape (Lower Lai)	10,159
Komonggu	4,874	Sau (Upper and	
Ono	2,474	Lower Sau)	8,473
Yabiyufa	4,464	Syaka (or Tchak) (Tale) ..	9,391
GAHUKU-BENABENA SUB-FAMILY	34,752	Laiap (Middle Lai)	10,657
Gahuku	11,390	Mae (Upper Lai, Ambum) ..	28,026
Asato	11,597	Maramuni	2,435
Benabena	11,765	Wale-Tarua	2,753
KAMANO-YAGARIA-KEIGANA SUB-FAMILY	60,651	Wapi (Wapi, Upper	
Kamano	31,342	Yuat)	2,500 estimated
Yagaria	14,294	Yandapu-Taro-Muritaka	
Keigana	8,443	(Lagaip-Laiagam)	8,017
Kanite	2,584	+ over 5,000 estimated	
Yate	3,988	Sirunki (Lake Sirunki) ...	4,839
FORE-GIMI SUB-FAMILY	28,756	+ over 3,000 estimated	
Fore	12,021	Waka (Wage)	app. 6,000
Gimi	16,735	Kandep-Aruni-Karintu	
HAGEN-WAHGI-JIMI-CHIMBU FAMILY	282,030	(Ka)	6,783
HAGEN SUB-FAMILY	90,239	+ over 1,000 estimated	
Hagen dialects	71,300	<i>Ipili Division</i>	
Medlpa	40,000	Ipili-Paiela	4,500 estimated
Tembagla	31,300	Porgera	2,500
Aua	439	Pagubela	2,000
Gawigl	18,500	Korombi	
WAHGI SUB-FAMILY	33,900	HULI SUB-FAMILY	54,000
Wahgi Dialects	33,900	Huli	35,000
Banz-Nondugl	16,100	Huliduna	18,100
Kup-Minj	9,500	MENDI-POLE SUB-FAMILY	63,757
Pukamigl-Andegabu	3,000	Mendi dialects	33,800
Kunjip	4,500	Kewa Pi	18,200
Kambia	800	Pole	6,046
JIMI SUB-FAMILY	16,100	Augu	3,100
Yoadabe-Watoare	3,200	Sau	2,611
Narak	6,150	WIRU SUB-FAMILY	11,541
Kandawo	6,750	Wiru	11,541
		DUNA FAMILY	app. 14,000
		Duna	14,000

¹ The figures referring to the number of the speakers of the individual languages are based on 1958-60 census figures.

Kamano-Yagaria-Keigana Sub-Family, than with members of its own sub-family. Yabiyufa is phonetically, and in many of its structural features, a Siane language; but lexically it is closer to Gahuku than to Siane. Ipili is structurally almost the same as Enga of the Enga-Ipili Sub-Family, but shows very strong lexical affinities to Huli of the Huli-Augu Sub-Family.

All the languages belonging to the Stock are tonal to some extent, with the greatest complexity of the tonal systems found in the Enga-Huli-Pole-Wiru and the Gadsup-Auyana-Awa-Tairora Families. Some correlation between tones and other suprasegmental elements, such as stress and length, appears to be present in most instances, and the tonal patterns show somewhat uncommon peculiarities. Intensive study is still required in this area. Of other phonemic features, the regional prevalence of the contrast between pre-nasalised and non-prenasalised stops over the contrast between voiced and voiceless stops deserves mentioning; also the common phonemic irrelevance of the distinction between stops, affricates and fricatives (e.g., *t - ts - s*), as well as the occurrence of lateral plosives and statistically very frequent glottal stops in individual families.

On the morphological level, the distinction between utterance-medial and utterance-final forms of the verb is a universal feature of the languages belonging to the Stock. This phenomenon, however, is also found in languages and language groups which are adjacent to, or at no great distance from, the area occupied by the Stock; and some of these may prove to be related. In most languages of the Stock, the utterance-medial forms appearing in those cases in which the subjects refer to the same actor in both parts of the utterance, are not identical with those utterance-medial forms which are met with in instances in which the subjects refer to different actors. Furthermore, several utterance-medial forms do, as a rule, occur in individual languages to denote different types of relationship between the actions referred to by the two verbs; e.g., simultaneity, successivity, duration, etc. Apart from having such utterance-medial verbal forms, the languages of the Stock are characterised by the fact that, almost universally, the verbal subject markers indicating the second and third persons of the dual and plural respectively, are identical. Other common features of these languages are the following: suffixes predominate over prefixes; gender and class systems are lacking; the verbal endings vary for person

(except in Duna); the negative is, in most languages, indicated by a verbal prefix; a dual number is universally present in the pronominal and verbal systems; and modal and aspect suffixes are added to utterance-final verbal forms. Furthermore, the indication of the pronoun object with the verb is very much the same in all languages of the Stock.²

Within the three Highlands Districts are found a small number of languages and language groups that do not belong to the East New Guinea Highlands Stock. Three of these are related to the Stock:

(1) Gandji-Karam-Maring, consisting of two closely related languages, Gandji and Karam-Maring, spoken on the north side of the Upper Jimi and on the Upper Kaironk in the Western Highlands and overlapping into the Madang District in the Asai and Simbai Valleys to the north of the Schrader and Mismarck Ranges, (very roughly, 10,000 speakers);

(2) Kutubi (Foi), around Lake Kutubu and in the Mubi Valley in the Southern Highlands (about 2,584 speakers);

(3) Mikaru, in the southwestern corner of the Eastern Highlands, overlapping into the Gulf District and into the eastern corner of the Southern Highlands (2,072 speakers counted; total number of speakers estimated to be about 4,000).

Calculated on the basis of shared percentages of basic vocabulary, the relationship of Gandji-Karam-Maring (10–15%) and Kutubu (10–13%) to the Stock is on the borderline between the order of stock and phylum.³ Borrowing may account for the somewhat higher percentages of shared vocabulary (as high as 19% and 17%, respectively) seen when these are compared with immediately adjacent languages of the Stock. The relationship of Mikaru is more distant (7–9% of shared basic vocabulary). These may be grouped with the East New Guinea Highlands Stock into an East New Guinea Highlands (Micro) Phylum (approximately 747,000 speakers).

The Pavia language overlaps from the Gulf District into the Eastern Highlands District along its southern border in the Aziana and Karimui Census Di-

visions. (Total number of speakers within the Eastern Highlands 187; total number of all Pavaian speakers under 2,000.) This language may prove to belong to the East New Guinea Highlands Phylum, but its relationship to the East New Guinea Highlands Stock is rather remote (between 3% and 5% of shared basic vocabulary).

The East New Guinea Highlands Phylum is distantly related to the Kate language family in the Huon Peninsula, and it also shows unmistakable relationship with the Ndani language in the Balim Valley in the Highlands of Netherlands New Guinea. Further study may show the existence of an enormous group of related languages, with close to 1,000,000 speakers, occupying a considerable portion of interior New Guinea.

The only languages found within the Highlands Districts which cannot be regarded as related to the phylum described are:

(1) a dialect of the Melanesian Atsera language of the Markham Valley, which is spoken within the borders of the Eastern Highlands by the 119 inhabitants of the village of Wampur on the eastern border of the Gadsup language area;

(2) the languages spoken by the "Kukukuku" natives living between the Aziana River and the eastern border of the Eastern Highlands. Three languages are met with in that area. Two of them, Wantakia (approximately 5,400 speakers) and Simbari (approximately 1,900 speakers) constitute a family. The third, Menyamnya, overlaps into the Eastern Highlands from the east (approximately 1,500 speakers within the Eastern Highlands). The degree of relationship, if any, between Menyamnya and the other two languages remains to be established. Wantakia and Simbari are both classifying languages (two classes), with highly complex verbal systems in which numerous prefixes and suffixes enter into the formation of individual verbal forms.

In the northwestern corner of the Western Highlands is an unknown language, spoken by perhaps 2,000 persons, which is known only by the Enga name "Kewa," meaning "stranger." The study of its characteristics and relationship is a task for the future.

To the south and southwest of Lake Kutubu, the perhaps interrelated Fasu (ca. 350 speakers) and Kaibu (ca. 400 speakers) languages seem to be quite different from Kutubu, and may not be related to, or members of, the East New Guinea Highlands Phylum.

Reported by S. A. WURM

² Characteristics of the individual families, sub-families, and languages are described in Capell.

³ According to Swadesh (op. cit.): Related languages sharing less than 12% of their basic vocabularies are said to belong to the same *phylum*. If the percentage of basic vocabulary items shared is between 4% and 12%, the languages are said to belong to the same *microphylum*.

More on Middle Pleistocene Hominids

SINCE PUBLICATION of F. Clark Howell's "European and Northwest African Middle Pleistocene Hominids" (CURRENT ANTHROPOLOGY [1960] 3:192-232), comments on it have been received from Frank B. Livingstone and George Cowgill. They are followed here by a reply from Howell that aims to bring the record and the discussion up to date.

Comments

By FRANK B. LIVINGSTONE

Howell's review of human evolution during the Middle Pleistocene was most thorough and timely; and his detailed outline of the geological contexts of the human fossils was particularly welcome. His article also seems to be ideal for CURRENT ANTHROPOLOGY and *CA* treatment, since it contains broad interpretations of the evidence, which for this period is quite fragmentary and open to a multiplicity of interpretations. Hence, the complete absence of discussion on this article is disappointing. There may be complete agreement among human paleontologists about the course of human evolution during the Middle Pleistocene; but I am sure this is not so for all physical anthropologists. As a physical anthropologist who is not an expert in human paleontology, I should like to disagree almost entirely with Howell's conclusions.

Briefly, the central disagreement concerns the number of different species of hominid which existed contemporaneously in the Middle Pleistocene. Howell concludes there were two species; I insist there was but one. Although this disagreement may seem trivial, particularly in view of the great number of different taxonomies of the Hominidae, its theoretical implications are enormous.

On the one hand, I maintain with Mayr (1950) and others that there has been no speciation among the Hominidae, but only phyletic evolution—i.e., different species may have succeeded one another in time, but at any one period of time only one species was extant. According to this viewpoint, the hominid lineage consists of three successive species which are usually called the Australopithecines, Pithecanthropines, and *Homo*. It should be noted that the term "speciation" is used to denote the branching process in evolution: a population speciates when it gives rise to two distinct descendant populations which are separate species. I would maintain that this did not occur in the hominid

lineage after the Australopithecine stage.

On the other hand, Howell maintains that in the Middle Pleistocene there existed two different hominid lineages of successive species and thus two different species. He places the European finds (Heidelberg, Swanscombe, and Steinheim) in one lineage; and the North African (Ternifine and Sidi Abderraham), Chinese (Choukoutien Lower Cave), and Javan (Trinil, Sangiran) finds in the other.

This disagreement rests, as do most scientific arguments, upon what one accepts as evidence either for or against placing fossils into one or two species; and in this case I think the disagreement is between the "bone-oriented" scientists on Howell's side and the "theory-oriented" ones on mine. The evidence on hand includes the scraps of bones that have been uncovered and the inferences we can make as to the environment and way of life of the organism. By a minute detailed analysis of the Ternifine and Heidelberg mandibles, Howell claims to show differences which are "fundamental," and therefore indicative of two different species. To Howell and most physical anthropologists, a bump here or a centimeter there is conclusive evidence for a specific difference. However, I maintain that according to our theories, assigning these mandibles to different species makes neither ecological nor evolutionary sense, and therefore the morphological evidence is not only inconclusive but should hardly be considered, beyond the fact that the two forms were generally similar and were what we would expect a Middle Pleistocene hominid to be like. I would go even further and maintain that we shall never infer or understand the course of human evolution by comparative anatomy or detailed analyses of the miserable scraps of bone which have been found. Only by considering these scraps as living, kicking animals who thus conform to all the general laws of population ecology and evolution will we ever understand our own ascent or descent.

In fairness to Howell, I should point out that his article is certainly not lacking in theory, and he does seem to ac-

cept the general proposition that spatial isolation is a necessary prerequisite for speciation among the higher mammals. Given spatial isolation, then the speed at which speciation occurs is, of course, dependent on other factors, particularly the amount of ecological divergence; but Mayr (1949) has estimated that it takes a minimum of several hundred thousand years, or roughly 30,000 human generations. If the diverging populations reestablish contact in the interim, they can mix and become one species, or one can become extinct, or they can occupy different ecological niches and by the phenomenon of character displacement become even more distinct (for a discussion of this concept in relation to the Hominidae, see Brown 1958).

It does not seem to me that there was sufficient time for the Middle Pleistocene hominids to speciate, assuming that both lineages derive from a generalized Australopithecine; and this view seems further justified by the small ecological divergence between them. Howell has shown that his two lineages were spatially isolated, one occupying Europe, and the other inhabiting an area stretching from North Africa to Java. But genetic divergence is partly a function of distance, so that similarity over such a great distance, coupled with diversity over such a short distance, would be very unexpected—particularly if there were no barriers. Howell does attempt to show that Europe was isolated from the rest of the hominid range; but with fluctuations in sea level it was not always isolated, and certainly not for the length of time required for speciation. But what seems to me to be even stronger evidence against speciation and isolation is that the North African and European groups shared the same tool traditions, while a different tradition existed in Asia.

In addition to the evidence against speciation, it seems to me that the theory of "no speciation among the hominids" renders the entire fossil record more understandable and comprehensible than does any other theory. It relegates to the trashpile of obsolescence many of the old problems of human evolution, and in this way is part of a common trend in science through which questions that were considered legitimate in one decade turn out to be illegitimate in the next. For example, two problems which have continually perplexed the "harrack" school of physical anthropology are: 1) the origin of modern racial differences; and 2) where *Homo sapiens* evolved and how he became distributed throughout the world in the last years of the Würm glaciation. As Coon (1959) has pointed out, the number of migrations required to place all human races

in their present areas is almost beyond imagination. But if one accepts the theory of no speciation, and thus the possibility that the various African, Asian, and European "Neanderthals" evolved into the present varieties of man, the whole problem disappears, and with it vanish the further problems raised by the increasing continuity of the archeological record in most of these areas, including France. This theory also makes sense of the great racial differences in the Old World vis-à-vis the New World, of which the explanation would be something like the following: The general selective pressures on a Paleolithic culture-bearing hominid were much the same everywhere, and during the Pleistocene produced the skeletal structure of *Homo sapiens* among all populations of the large, widespread, polytypic lineage. On the other hand, climatic and other lesser environmental differences were, at the same time, tending to produce the divergences in skin color, etc., which are characteristic of human groups today. As a result, one has structural similarity and physiological divergence. I have used only two problems as examples; but to my mind, there is almost nothing in the fossil record that cannot be explained in terms of the theory of "no speciation among the hominids." Hence, I would welcome further criticism or discussion of this—to me—unassailable theory.

By GEORGE COWGILL

In a recent issue of *Science* (Vol. 131, No. 3412, p. 1511) I see that Kurtén finds that pollen at Choukoutien indicates a glacial, rather than interglacial, climate; and that the fauna suggests late Mindel (Elster) rather than Riss (Saale). Evidently this pollen and fauna is associated with "*Homo pekinensis*."

If this is so, it seems that Pekin Man would be roughly contemporaneous with Ternifine and with Java Man of the Trinil beds, all being late Early Middle Pleistocene. It seems further that in this case there would be no evidence of pithecanthropine (in Le Gros Clark's terms) peoples anywhere in the world later than Early Middle Pleistocene, save perhaps for the jaws from Littorina Cave and Rabat. Is it possible, then, that by the time of Swanscombe and Steinheim there were no more populations of the type represented by Ternifine, the Trinil Beds, and the early Choukoutien peoples left anywhere; so that these two broad groups of hominids belong to different time periods without any appreciable overlap?

If Kurtén's dating is acceptable, it seems to make the Far Eastern cultures rather less conservative than had been thought; and, it brings evidence for the use of fire down to a quite early date.

Reply

By F. CLARK HOWELL

Cowgill and Livingstone have raised significant points in regard to my paper. The writer welcomes and is grateful for this and any other constructive criticism. Their respective comments can only be discussed briefly here.

The human remains from Choukoutien have been generally agreed to be of Middle Pleistocene age, although the more precise stage within that extended range of time has never been clearly settled. In general, I have followed the studies of Teilhard de Chardin (1941, and earlier) and the excellent critical syntheses by Movius (1944, 1955) concerning the Pleistocene stratigraphy of northern China. The latter author suggested a broadly mid-Middle Pleistocene age on the basis of both mammalian fauna and comparative stratigraphy. In terms of comparative stratigraphy and regional succession, this would apparently correspond with the Great Interglacial stage of the European Pleistocene sequence.

Several lines of evidence now indicate that this dating is somewhat too young. Kahlke and Hu (1957), in their interesting analysis of the Megacerine deer from the Pleistocene of China, concluded that the fauna at Choukoutien best equated with a time interval equivalent to the terminal Elster Glaciation or the immediately subsequent initial phases of the Elster/Saale (= Mindel/Riss) Interglacial. Kurtén's (1956, 1957a, b) fine study of the ecological replacement of the giant hyena (*H. breivrostris*) by the spotted hyena (*Crocuta crocuta*), which occurred at Choukoutien Locality I between the lower brecciated horizons (Zone II) and the upper travertine horizons (Zone I), points in the same direction. Kurtén originally thought this replacement occurred during the Cromerian stage—a conclusion I had criticized because the European evidence, at least, favored a late Elster Glacial date for the replacement. Kurtén (1960), in a superb analysis of the Lower and Early Middle Pleistocene mammalian faunas of Europe, has recently altered his opinion to one in full agreement with my own. He has also noted that the "relative numbers of living and extinct species of mammals are about the same (Choukoutien about 44 per cent living species, Elster II about 54 per cent)," compared with the much higher percentage (ca. 78 per cent) from the later Middle Pleistocene (Kurtén, 1959a). Cowgill refers to a further significant fact which buttresses this conclusion: a pollen spectrum obtained from matrix adhering to a specimen of *Megaceros*

pachyosteus from Locality I. Kurtén (1959a) states that this evidence indicates "a somewhat cooler climate than that of the present day in the Peking region," the ecology being then nearer to that of the northern coniferous-temperate steppe border zone than to the temperate-park steppe zone and especially the presently adjacent subtropical evergreen forest belt.

As a consequence of these very pertinent investigations, the hominid skeletal remains from Choukoutien Locality I should be referred to the terminal phases of the earlier Middle Pleistocene, i.e., correlative with late Elster (Mindel) of the European succession. This population sample is thus broadly equivalent in time with the Ternifine people, but apparently somewhat younger than the isolated individual represented by the human mandible from Mauer. All are considerably older than the European human population represented only by skeletal remains from Swanscombe, Steinheim and probably Montmaurin, belonging to the middle to late Great Interglacial (Hoxnian or Holsteinian) stage.

The evidence is uncertain, due to enormous gaps in the fossil record, but it may very well be that Cowgill is correct in suggesting that "by the time of Swanscombe and Steinheim there were no more populations of the type represented by Ternifine, the Trinil Beds, and the early Choukoutien peoples." This is probably the most economic hypothesis at present and surely fits the available evidence (notwithstanding the fact that there are certain similar characteristics in the dentition and mandible in the fragmentary human remains from Sidi Abderrahman and Rabat in Morocco, which are of later Middle Pleistocene age).

There is now no positive evidence favoring my earlier conclusion that there were distinct contemporaneous *allopatric* species of mid-Middle Pleistocene hominids. Livingstone has questioned this conclusion on quite different, and essentially theoretical, grounds. So long as the dating of Choukoutien was mid-Middle Pleistocene, however, I would maintain, as in my review article, that the evidence led inevitably to the conclusion that distinct *allopatric* species were involved. This is another clear indication of the unity of paleoanthropological studies, which depend on a close integration of modern evolutionary theory with Pleistocene geological, ecological, prehistoric archeological and human paleontological approaches.

Human evolutionary studies are still greatly hindered by a taxonomic morass which is seriously in need of revision. It is worth citing here several prominent and recent (within the last decade) sys-

tems to make this point clear. In Le Gros Clark's (1955) scheme, three distinct and successive, but overlapping genera (*Australopithecus*, *Pithecanthropus*, and *Homo*) are distinguished within the Hominidae; and (at least) two species are recognized within each of these genera. At another extreme is the scheme proposed by Mayr (1950) which contains three successive species (*transvaalensis*, *erectus*, and *sapiens*). All sorts of intermediate classifications have been proposed, or are implicit in the writings of several current investigators; these usually lack the necessary diagnoses, and often there has also been a lamentable lack of attention to the international rules of nomenclature. In my opinion, both Le Gros Clark's and Mayr's schemes have serious drawbacks and fail to consider some of the significant evidence essential for a realistic and workable taxonomy of the Hominidae. At least two genera with several species in each are recognizable within the Hominidae on grounds which would be acceptable to most workers concerned with mammalian evolutionary problems. The matter cannot be discussed here, but is considered further in a forthcoming book (Howell 1961).

The classification proposed by Mayr is evidently accepted by Livingstone, to judge from his letter. Mayr (1950: 113) interpreted certain fossil human remains (Steinheim, Swanscombe, etc.) as "remains of populations of *Homo*," which he evidently thought were subspecies within *Homo sapiens* (p. 115). He also considered that "Java and Peking man are sufficiently distinct from modern man so that they have to be considered a separate species, which must be called *Homo erectus*." If the available evidence favored the contemporaneity of these hominids, there was obviously coexistence of two separate species, following Mayr's and Livingstone's own arguments. In this respect, Livingstone completely contradicts himself when he accepts Mayr's point of view and then states that the fundamental "disagreement is between 'bone-oriented' scientists on Howell's side and 'theory-oriented' ones on mine." Few workers in human biology, who are even faintly cognizant of the tremendous advances made in the fields of systematics, genetics, and modern evolutionary biology, could fail to appreciate the significance of the modern species concept, as presented especially in Ernst Mayr's (1947, 1948, 1949, 1957, 1958, 1959) numerous and stimulating papers. However, the difficulty of readily applying the biological species concept to fossil populations is quite another matter, as most paleontologists fully recognize (cf. symposium: The species concept in paleontology. System-

atics Association, London, Publ. No. 2. 145 pp. 1956). I still maintain, as I said in my article, that Simpson (1943, 1951) has made the most useful positive contribution toward the solution of this dilemma.

I stress this point only because in this particular case the basis of the difficulty is not at all a consequence of difference in background and orientation. It is a result of insufficient evidence and inadequate methods for determining the relative ages of Pleistocene hominids within local stratigraphic successions, and particularly for ascertaining the inter-relationships of such widely-separated regional successions. Livingstone's main points are in essential agreement with my own present viewpoint, although I hesitate to reach conclusions on the basis of *a priori* reasoning. As Livingstone stresses, there are certain difficulties with the view of contemporaneous but distinctive allopatric species of hunting and gathering hominids in the Middle Pleistocene. I was wrong in this respect, but for reasons quite different from those raised by Livingstone.

Livingstone maintains, as have certain other workers (especially Dobzhansky 1944; Mayr 1950), that hominid evolution, at least after the australopithecine stage, was characterized by phyletic evolution (successive species), whereas speciation (branching evolution) was nil. In support of this interpretation, he mentions two main points: (1) shortness of time (at least several hundred thousand years, or $\pm 30,000$ human generations, are required for speciation to occur) and (2) lack of ecological divergence. That selective pressures on a Paleolithic "culture-bearing hominid" were generally similar is apparently an additional factor. He may well be right; and at present there is no direct evidence to controvert this position. However, there are certain difficulties which should be made more clear.

The question of time needs to be approached in a more empirical manner. Kurtén's (1959b) work on rates of evolution in fossil mammals, particularly Pleistocene faunas, indicates several quite distinctive rate patterns, ranging from very high to very low. Differentiation to the species level could occur in the former case in some 25,000 years, and in the latter case in some 700,000 years. This is no mean difference, and it is probably misleading to suggest an approximately intermediate or median value.

For brain volume in the post-australopithecine phases of hominid evolution, Kurtén has plotted a very high rate of initial change followed by gradual retardation. With such a (perhaps initial) potential for high rates, it is altogether

likely that speciation *could* occur within the initial primary dispersal of early tool-making hominids, given appropriate conditions of isolation. The extensive ecological changes, throughout much of the Old World, brought about as a consequence of the climatic deterioration in far northern and southern latitudes in the earlier Middle Pleistocene, could surely have served in some instances as appropriate isolating barriers. Hence, conditions conducive to speciation were undoubtedly present even though the sparse fossil record fails to provide conclusive proof that such actually occurred.

Surely it is also incautious to infer a broad similarity of selective pressures on early hominid populations. All the evidence accumulated by paleoanthropological studies of Middle Pleistocene human occupation sites, as well as earlier and more traditional pre-historic studies, seems to indicate that the cultural capabilities and technological levels of these groups were strikingly different from anything known among *Homo sapiens* populations, either extant or extinct. Anthropologists—some of whom have been traditionally interested in culture, its components, and their manifestations—have generally failed to appreciate Pleistocene paleoanthropological evidence and its bearing on the evolution of human behavior. Prior to the appearance of dominant *Homo sapiens* populations in the later Upper Pleistocene it is extremely difficult to make meaningful sense of cultural levels in terms of any extant comparative "ethnological reality." Considerable evidence suggests (admittedly indirectly) that human behavior and capabilities throughout the Middle Pleistocene were considerably dissimilar from those of *Homo sapiens*. The utmost circumspection should be employed in posing hypotheses based on the behavior patterns and elaborate technological and cultural adjustments representative of *Homo sapiens* populations over the past thirty-five thousand or so years. In this respect, the study of the present as a guide to the past has definite and severe limitations, which are not yet well appreciated by human biologists (including students of human paleontology, myself not excepted).

The species *Homo erectus* is essentially of early Middle Pleistocene age. The reference of the Mauer mandible to this species is difficult, as I stressed previously; and is even contradicted by certain important characters of its dentition and mandibular morphology. Because of the incomplete nature of the remains, they are best regarded only as *Homo* sp.; and it would be best to follow the same procedure with the hominid skeletal remains from

Sidi Abderrahman and Rabat. In recent years different writers have variously referred the remains from Steinheim and Swanscombe (and those more recently found at Montmaurin) to either *Homo neanderthalensis* or *Homo sapiens*. I merely sidestepped the issue of taxonomy in my paper. There are arguments for and against both points of view, although I slightly favor the former. A distinct species, *Homo steinheimensis* by priority, would be a temporary solution to the problem, with the species to be eventually relegated to synonymy if necessary, and if and when additional and better-preserved specimens are discovered. In this scheme, two successive species of hominids would be distinguished for the Middle Pleistocene range of time. The available data would thus support Livingstone's theoretically-based assertion that after the australopithecine stage there was no speciation, but only phyletic evolution within the genus *Homo*.

The question of the coexistence of sympatric species of hominids is still open in two instances. The first is at Swartkrans (Transvaal), where abundant remains of a large australopithecine (*Paranthropus*) are found with a few fragmentary remains of another and certainly distinct hominid (referred by J. T. Robinson to *Telanthropus*). Whether this indicates the displacement of the small, earlier australopithecine (*Australopithecus*) by the larger, dominant later form (*Paranthropus*), as has been suggested by S. L. Washburn (personal communication), or the presence with the latter of an early species of *Homo*, is still an unresolved issue. The second instance is the Putjangan beds (central Java) which have yielded early *Homo erectus* remains, as well as several fragments of mandible and isolated teeth of another form, *Meganthropus* of von Koenigswald. In complete agreement with J. T. Robinson, I am convinced that "*Meganthropus*" represents a large, late form of australopithecine (*Paranthropus*). However, the Putjangan lacustrine black clays are nearly two hundred meters thick and obviously cover a very considerable range of time, extending perhaps from the late Villafranchian and surely into the earlier Middle Pleistocene. The stratigraphic relationships of the hominid fossils are not definitely known. The specimen briefly described by Marks (1953) was evidently weathered out from the fossiliferous cemented conglomerate toward the top of the Putjangan series designated *Grenzbank* by von Koenigswald (1940). The latter is further overlain by some three to five meters of black clays followed by some thirty meters of

Kabuh beds, conglomeratic and tuffaceous sandstones with characteristic Trinil stage fauna. Whether the remains of the earlier form of *Homo erectus* derive from the same or a different horizon in the series as those of "*Meganthropus*" is still unknown.

Proper discussion of Livingstone's last point, concerning the origin of *Homo sapiens* and the age and significance of modern racial differences, would require more space than is available here. Broadly speaking, some paleontological evidence supports his thesis, which was propounded by a variety of workers in the past, that various African, Asian, and European hominids, sometimes referred to as "Neanderthaloid" peoples, were possible forerunners of various geographic races within the polytypic species, *Homo sapiens*. Evidence that this was probably true of certain western Asian Neanderthal (*suo loco*) populations is becoming increasingly clear. However, the evidence is equally clear that this was not necessarily the case in general, as witness the European Neanderthal populations of the Early Last Glacial which were replaced by certain dispersing early *Homo sapiens* (*sensu strictu*) populations. (In this respect, it is surely misleading to speak of "the increasing continuity of the archeological record," which in France—a case cited by Livingstone—certainly demonstrates a non-sequence and marked cultural differences in all sites where any real succession is present.) I think it very probable that in certain geographically peripheral areas, such as western and southern Europe, southern Africa, and southeast Asia (including Indonesia), replacement of this sort did actually occur. Hence, populations such as the Classic Neanderthals, Broken Hill-Saldanha folk, and the Solo peoples, represented semi-isolated and special evolutionary manifestations in marginal areas, compared with human populations of the earlier Upper Pleistocene in more centrally situated continental environments.

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Two New Projects:

The Current Anthropology Dictionaries

AT THE 6TH INTERNATIONAL CONGRESS of Anthropological and Ethnological Sciences in Paris (July 31 to August 6, 1960), a special meeting was held to consider two projects. Dr. Wolfgang H. Lindig, of Germany, presented a plan for a *Dictionary of Anthropology*, and Dr. Grete Mostny, Chile, set forth a proposal for a *Multilingual Glossary of Anthropological Terms*. Those present at the meeting decided that CURRENT ANTHROPOLOGY, as a journal of worldwide distribution, was the best medium through which contributions to

both these projects could be gathered and published, and through which the co-operation of anthropologists throughout the world, which is essential for their realization, could be secured.

The *Glossary* and the *Dictionary*, although not internally related, complement each other; and it is hoped that they will not only become useful tools for anthropological teaching and research, but will also lead toward standardization of terms for the same objects or concepts.

A Dictionary of Anthropology (DA)

The *Dictionary of Anthropology* is to include articles on (1) terms and concepts used in anthropological fields, including prehistory, linguistics, physical anthropology, and ethnology; (2) designations and descriptions of prehistoric and modern cultures; and (3) biographies of anthropologists.

Although important anthropological concepts may be found in several dictionaries—such as Eberts *Reallexikon der Vorgeschichte* (15 volumes), Funk and Wagnall's *Standard Dictionary of Folklore* (2 volumes); *Wörterbuch der Soziologie* (1 volume) and the UNESCO *Dictionary of Sociology* (in press)—which have been published in related disciplines, the existing dictionaries of anthropology were written for laymen (and usually by laymen) and are of little use to specialists. A dictionary of anthropology for anthropologists is long overdue. Since the field of anthropology is so extensive, co-operation among specialists from all parts of the world is required to bring such a dictionary into existence. CURRENT ANTHROPOLOGY is an ideal means whereby these specialists can be contacted, their articles printed, and their comments received and integrated. All Associates, therefore, are asked to collaborate by writing articles for publication in a CA "Dictionary" section of the journal. After

publication, any Associate may comment on the articles, and their comments will be printed in subsequent issues. Following a revision and synthesis of the articles and comments, it is planned to publish them as a book; but this project is secondary.

Publishing the articles in CURRENT ANTHROPOLOGY can overcome the difficulties, involving limitations of space and time, which arise in planning a multi-volume dictionary. Limitations of space—i.e., whether particular subjects should or should not be included—may be illustrated by the problem of including terms relating to classical archaeology or indology. An Associate who is willing to write articles pertaining to these fields is welcome to do so. The articles will be published, like articles on other fields of anthropology. Perhaps other archaeologists or indologists will follow suit, and thus the *Dictionary* can be built up and expanded at the will of the collaborators. As for the time limit, the date for completion of the *Dictionary* is not of great importance; in fact, the steady growth of anthropological knowledge may prevent its ever being completely finished. In the meantime, the articles will appear in CURRENT ANTHROPOLOGY, whose editor will, if possible, make available extra sheets of the articles, which can be cut

out and filed. An index of the terms published each year will appear annually in the last number of the journal.

Examples of articles for the *Dictionary* are printed on the next page. No article should be longer than three or at most four double-spaced typewritten pages. Line drawings and/or photographs should be included whenever necessary. Articles should be written in English; but ways will be found to overcome any difficulties presented by translation.

These will not be review articles. Complex terms should be split up into single concepts—e.g., the article on "chronology" should mention "dendrochronology," "radiocarbon dating," "lexicostatistics," etc., but these terms will be treated in separate articles. Authors should give particular attention to clearing up confusions of technical terms and misunderstandings of concepts. A bibliography of the most important publications on the subject should be included. The author's name will appear at the end of each article.

The Editor of the *Dictionary of Anthropology* is Dr. Wolfgang H. Lindig, Mainz/Rhein, Jägerstrasse 24, Germany. Associates who are willing to contribute to the *Dictionary* should tell him, as soon as possible, which articles they are prepared to write; and the titles of these will be published to avoid duplication.

A Multilingual Glossary of Anthropological Terms (MG)

The *Multilingual Glossary of Anthropological Terms* will bring together, in four or more languages, all the words and *termini technici* needed for their research by anthropologists (including physical anthropologists, ethnologists, archaeologists, etc.). The *Glossary* will

consist of two parts:

1. A *topical section* with 24 chapters, the proposed outline of which is shown in pages 134-35. In each chapter, the entries will be arranged from the general to the particular. Simple drawings of objects and their component parts, as

shown on page 135, will be added to facilitate understanding. Each term will be given in all of the languages used in the *Glossary*.

2. A *lexicon* with four or more independent alphabetically-arranged vocabularies, each headed by one of the

languages, with each term translated into the other languages used. Preliminary examples are printed below of vocabularies in English, French, German, and Spanish, the languages now used in the *Glossary*; others may be added, to the extent permitted by the size of the pages of CURRENT ANTHROPOLOGY. At present, the maximum number of languages seems to be six.

Originally the effort of a single person, Grete Mostny, the project was expanded at the Paris Conference, where a number of specialists agreed to col-

laborate. More participants will be needed as the *Glossary* becomes larger, and more languages are added. Lists of words will be sent to collaborators for completion and correction, and then returned to Dr. Mostny, who will forward them to CA for publication. In this way, many anthropologists can read, correct, and discuss the terms; and it is hoped that this procedure will standardize anthropological terminology. Besides highly-specialized terms, the *Glossary* should include words frequently used by anthropologists, even if found

in ordinary dictionaries. Thus, persons whose knowledge of a particular language is more or less rudimentary will not be obliged to refer to dictionaries other than this multilingual glossary.

The Editor of the *Multilingual Glossary of Anthropological Terms* is Dr. Grete Mostny, Museo Nacional de Historia Natural, Casilla 787, Santiago, Chile. Associates who are willing to collaborate on the project are asked to write her directly for further information.

SAMPLE ENTRIES FOR THE DICTIONARY OF ANTHROPOLOGY

Note that one example concerns a people; the other, an institution or concept. Discussion of other categories, and of the way these two examples are handled here, is wanted.

Seri

North American Indian tribe on Tiburón Island in the Gulf of California and adjacent parts of Sonora (Mexico). In 1960, they numbered approximately 160 individuals. The Seri speak a Hokan language. Like their closest ethnic relatives, the now-extinct Yuman tribes of central and northern Baja California, they are hunters and gatherers who catch fish and sea-turtles, hunt deer and mountain-sheep in the sierras of the island, and gather mesquite and cactus-fruit (*pitahaya*). They make crude pottery, a trait probably acquired by diffusion from their agricultural neighbors, the Pima. Hunting and gathering techniques and procedures (ceremonial deer hunt, "second harvest"); technology (boat-shaped rush rafts, two-pronged fishing spear); religious ideas (shamanism); social organization (gerontocratic principles); physical characteristics (dolichocephaly); and prehistoric links (tree-burial) indicate that the Seri represent a survival of an old hunting and gathering substratum, which in prehistoric times spread over the entire western half of the North American arid Southwest.

WOLFGANG H. LINDIG
BIBLIOGRAPHY: McGee, W. J., *The Seri Indians*, Bureau of American Ethnology, 17th Annual Report, Washington, 1898; Kroeber, Alfred L., *The Seri*, Southwest Museum Papers, 6, Los Angeles, 1931; Lindig, Wolfgang H., "Die Seri," *Internationales Archiv für Ethnographie*, vol. 49, Leiden, 1960; Lindig, Wolfgang H., "Über die Bestattung bei den Seri," *Baessler-Archiv N.F. Band VII*, Berlin, 1959.

Gerontocracy

(Greek: *geron* = old men and *kratein* = to rule), rule of the old men. In almost every society old age commands respect, lends authority, and enhances prestige. The influence of the older men—and less frequently, women—however, may not be evident in the formal government of the group, for the older members sometimes take a less active part in the normal daily life of the community. Yet, as the great storehouse of knowledge and tradition they exercise an enormous influence on the younger members of the society, and their authority and guidance in ceremonial, political, and juridical matters is frequently pronounced.

The mental and psychological superi-

ority of the old is due to and explained by the inexperience of the younger generation, generally speaking. The focus of social life necessarily lies on that age in which the mental and physical powers of man are balanced best and in which the accumulated experiences of a life-time enable an optimum adaptation to the demands of life. The young are always in need of assistance and guidance by the more experienced elders. Since a systematic education does not exist in most tribal communities, this adaptation to life is much more dependent upon direct personal experiences than in our own civilization, where theoretical images attained by reading and reasoning take the place of direct personal experiences. Furthermore, the absence of the knowledge of writing is of considerable importance in this context, for writing enables man to take down and accumulate his experiences and thus make them available for the next generation, so that the continuous trial-and-error process can be disposed of. Another important cause for the existence of gerontocratic principles in tribal societies lies in the hardship of life among those peoples. Their whole life is, contrary to the belief of many romantic Europeans, an endless struggle for existence; life with them is less hygienic, and the frequently one-sided nutrition is often insufficient. Under these conditions people grow older faster than in our civilization. Considering the penuriousness of his life, an older man, once he has attained a certain social status within his community, is unwilling to let younger men take away this position from him. If he is physically unable to hold his position, he devises ways and means whereby his status of superiority can remain untouched. In some societies the old men's authority is so great that they even constitute a dominant ruling class whose members control all communal activities.

All these factors are of great importance for the origin and functioning of gerontocratic institutions and principles. Even so, societies with such institutions show different patterns. As a rule, the government lies in the hands of the older men, i.e. those between 30 and 50 years of age: they decide all questions of importance, administer justice, organize ceremonies, and regulate the movements of the group. Apart from these political, ceremonial, and juridical powers, they frequently demand a number of privileges, such as having the right to certain choice items of food, in

which case these are declared taboo for the younger members of the group. Sometimes the elders enjoy sexual privileges with the unmarried young women of the group or claim the right to have several wives, whereas the young men may possess only one wife. The whole power and prestige enjoyed by the older members of a community unfolds itself during the initiation ceremonies (q.v.), i.e. the ceremonial introduction of the younger members into the society of the adults. But the elders do not always constitute a separate group within a community.

In certain parts of Melanesia, where there is no organized form of government, social affairs are conducted by the older men generally. In particular, there may be a few old men whose knowledge of magic and traditional lore gives their decisions great weight and authority. Their power, however, is not absolute; they exercise it, not by command, but by advice; and they have no means to enforce their decisions. Where social institutions such as secret societies play an important role in the life of a tribal society, membership to elders' councils depend on a man's position in such associations. Usually, as in many parts of Melanesia, these societies are hierarchically graded and the higher ranks can be attained only through age and wealth. Generally speaking, the qualification for supreme political power is not age alone, but knowledge of magic and tradition, sometimes wealth or ability as a leader in war or debate. These qualifications add considerably to the authority of the old men, depending on the general cultural pattern of the particular society.

There is no uniform pattern of gerontocracy but rather a general tendency, the direction of which is determined by local circumstances. Gerontocratic forms of government usually develop in small tribes where there is no strong central authority and where complicated religious or magical ceremonies are observed. Gerontocracy occurs most frequently in Melanesia and Australia, and is also found in certain parts of Africa and America.

WOLFGANG H. LINDIG
BIBLIOGRAPHY: Schurtz, Heinrich, *Altersklassen und Männerbünde*, Berlin 1902; Lowie, Robert H., *Primitive Society*, New York 1920; Rivers, W.H.R., *Social Organization*, London 1924; Thurnwald, Richard, *Die menschliche Gesellschaft in ihren ethno-soziologischen Grundlagen*, vol. 2, Berlin 1932.

SAMPLES OF THE MULTILINGUAL GLOSSARY

*Note that the glossary can be published in CA with as many as six languages on the page.
Which should be the two additional languages?*

ENGLISH	SPANISH	FRENCH	GERMAN	ADDITIONAL LANGUAGES
adze	azadón (m)	bêche (f)	Hauc (f), Hacke (f)
amber	ambar (m)	ambre (m)	Bernstein (m)
arrow	flecha (f)	flèche (f)	Pfeil (m)
arrow-head	punta de flecha (f)	pointe de flèche (f)	Pfeilspitze (f)
awl	punzón (m), lezna (f)	poinçon (m)	Ahle (f)
axe	hacha (f)	hache (f)	Axt (f)
axle	eje (m)	axe (m)	Achse (f)
axilla	axila (f)	aisselle (f)	Achsel (f)
bark	corteza (f)	écorce (f)	Rinde (f)
basket	canasto (m), cesto (m)	pannier (m)	Korb (m)
basketry	cestería (f)	vannerie (f)	Korbflechterei (f)
beak	pico (m)	bec (m)	Schnabel (m)
beaker	copa (f)	coupe (f)	Becher (m)
beam	viga (f)	poteau (m)	Balken (m)
bedrock mortars	pedra de tacitas (f)	pierre à cupules (f)	Schalenstein (m)
bituminous coal	hulla (f)	houille (f)	Steinkohle (f)
blade	hoja (f)	lame (f)	Klinge (f)
blow-gun	cerbatana (f)	sarbacane (f)	Blasrohr (n)
borer	taladro (m), barrena (f)	foret (m)	Bohrer (m)
breccia	brecha (f)	brèche (f)	Breccie, Brekzie (f)
buckler	escudo (m)	bouclier (m)	Schild (m)
bulbar face	cara de fisión (f)	face d'éclatement (f)	Unterseite (f)
bulbar scar	écaillage (f)	Schlagmarke (f)
burin	buril (m)	burin (m)	Grabstichel (m)
cache	escondite (m)	cache (f)	Versteck (n)
calf	pantorilla (f)	mollet (m)	Wade (f)
carinated	carenado	caréné	gekielt
carrying-pole	balancín (m)	balancier (m)	Tragstange (f)
ceiling	cielo raso (m)	plafond (m)	Decke (f)
cell	célula (f), celda (f)	cellule (f)	Zelle (f)
chain	cadena (f), urdimbre (f) (text.)	chaîne (f)	Kette (f)
chest	pecho (m)	poitrine (f)	Brust (f), Brust- kasten (m)
chipping-tool	formón (m)	retouchoir (m)	Retouchierstab (m)
chisel	cinzel (m)	ciseau (m)	Meissel (m)
chopper (prehist.)	partidor (m)	hâchoir (m)	Hackstein (m)
cleavage plane, see bulbar face
cliff	acantilado (m)	falaise (f)	Kliff (n)
club	rompe-cabeza (m)	casse-tête (m)	Keule (f)
coarse grained	de grano grueso	de gros grain	grobkörnig
coiled basketry	cestería (f) en ajuja espiral	roue (f) de cordage	Spiralwulst- geflecht (n)
core	núcleo (m)	nodule (m)	Kernstück (n)
couvade	cuvada (f)	couvade (f)	Männerkindbett (n)
cross stitch (text.)	punto cruz (m)	point croisé (m), point de croix	Kreuzstich (m)
crucible	crisol (m)	creuset (m)	Schmelztiegel (m)
dentition	dentición (m)	dentition (m)	Zahnung (f)
digging stick	estaca para excavar (f)	bâton de fouille (m)	Grabstock (m)
dorsal face	cara dorsal (f)	face dorsal (f)	Oberseite (f)
dugout	piragua (f)	pirogue (f)	Einbaum (m)
endscraper	raspador terminal (m)	grattoir (m) sur bout de lame	Endkratzer (m), Endschaber (m)
engraver's chisel, see burin
eyeball	niña del ojo (f), globo del ojo (m)	globe de l'oeil (m)	Augapfel (m)
eyelash	pestaña (f)	cil (m)	Wimper (f)
eyelid	párpado (m)	paupière	(Augen-) lid (n)
eye-socket	órbita (f)	orbite (f)	Augenhöhle (f)
fiber	hebra (f), fibra (f)	fibre (f)	Faser (f), Fiber (f)
fine grained	de grano fino	de grain fin	feinkörnig
fingerprint	impresión (f) digital	empreinte (f) digitale	Fingerabdruck (m)
fire drill	taladro de fuego (m)	tourniquet de feu (m)	Feuerbohrer, -quirl (m)
firing (cer.)	cochura (f)	cuisson (m)	Brennung (f)
fishglue	colapez (f)	colle de poisson (f)	Fischleim (m)
fish-hook	anzuelo (m)	hameçon (m)	Angel (f), Angel- haken (m)
fishing line	sedal (m)	ligne (f)	Leine (f), Angelschnur (f)
fist	puño (m)	poing (m)	Faust (f)

ENGLISH	SPANISH	FRENCH	GERMAN	ADDITIONAL LANGUAGES
flake	astilla (f)	éclat (m)	Abschlag (m)	
flaring rim (cer.)	borde de menor a mayor	col évasé (m)	ausladender Rand (m)	
fleece	vellón (m)	toison (m)	Vlies (n)	
fore-piece	ante-asta (f)	hampe (f)	Vorsatzstück (n), Vorscheit (m)	
glazed	vidriado	vitriifié	glasiert	
grater	raspador (m)	grattoir (m)	Kratzer (m)	
grave goods	ajuar funerario (m)	mobilier funéraire (m)	Grabbeigaben (f)	
gravel	grava (f)	gravier (m)	Schotter (m)	
graver, <i>see</i> burin				
groove	ranura (f)	rainure (f)	Rille (f)	
hammerstone	percutor (m)	percuteur (m)	Schlagstein (m)	
hand-axe	hacha (f) de mano, cuña (f) de mano	coup de poing (m)	Faustkeil (m)	
handle (cer.)	asa (f)	anse (f)	Henkel (m)	
handle	mango (m)	manche (m)	Griff (m)	
hatched line	línea (f) estriada	ligne (f) striée	schrattierte Linie (f)	
hemp	cañamo (m)	chanvre (m)	Hanf (m)	
hinge	gozne (m)	pivot (m)	Angel (f)	
hub	cubo (m)	moyeu (m)	Nabe (f)	
instep	empeine (m) del pie	cou (m) de pie	Spann, Rist (d. Fusses) (m)	
ivory	marfil (m)	ivoire (m)	Elfenbein (n)	
javelin	azagaya (f)	sagaie (f)	Speer (m)	
jaw	quijada (f)	mâchoire (f)	Kiefer (m)	
knee	rodilla (f), codo (fig.) (m)	genou (m)	Knie (n)	
Knuckle-duster	manopla (f)	coup de poing (m)	Schlagring (m)	
lance head	punta de lanza (f)	pointe (f) à cran	Lanzenspitze (f)	
laurel leaf point	punta (f) de lanza en forma de hoja de laurel	feuille (f) de laurier	Lorbeerblattspitze (f)	
lid	tapa (f)	couvercle (m)	Deckel (m)	
link	eslabón (m)	anneau (m)	Glied (n)	
lintel	dintel (m)	linteau (m)	(Für-) Sturz (m), obere Schwelle (f)	
lip	labio (m)	lèvre (f)	Lippe (f)	
loom	telar (m)	métier (m)	Webstuhl (m)	
lower jaw	mandíbula (f)	mandibule (f)	Unterkiefer (m)	
mace	maza (f), clava (f)	massue, masse (f)	Keule (f)	
maxillary, <i>see</i> upper jaw				
microlith, <i>see</i> pigmy flint				
nave, <i>see</i> hub				
nucleus	núcleo (m)	noyau (m)	Nukleus (m)	
one-ply	de un cabo (m)	d'un brin (m)	einelementig	
ore	mineral (m)	minéral (m)	Erz (n), Mineral (n)	
Pan pipe	flauta de Pan (f)	flûte (f) de Pan	Panpfeife (f)	
pebble	guijarro (m)	galet (m), caillou (m)	Kiesel (-stein) (m), Knollen (m)	
percussion bulb	bulbo (m) de percusión	bulbe, cone (m) de percussion	Schlagbuckel (m)	
pick (text.), <i>see</i> weft				
pick	pico (m)	pic (m)	Pickel (m)	
pick-axe	piocha (f), picota (f)	pioche (f)	Pickel, Pickaxt (f)	
pigmy flint	microlito (m)	microlith (m)	Zwergflint (m), Mikrolith (m)	
pillar	pilar (m)	pilier (m)	Pfeiler (m)	
pincers	pinza (f)	pince (f), pincette (f)	Pinzette (f)	
pit	pozo (m)	puit (m)	Grube (f), Schacht (m)	
pitcher	cántaro (m)	cruche (f)	Krug (m)	
plain weave	ligatura llana (f), ligatura de cañamo	armure toile (f)	Leinenbindung (f)	
plane	cepillo (m)	rabot (m)	Hobel (m)	
plaster	yeso (m)	plâtre (m)	Gips (m), Bewurf (m)	
pliers	tenazas (f)	tenaille (f)	Zange (f)	
plow	arado (m)	charrue (f)	Pflug (m)	
ply	cabo (m)	brin (m)	Element (text.) (n)	
potter's wheel	torno de alfarero (m)	tour (m)	Töpferscheibe (f)	
pottery	alfarería (f)	poterie (f)	Töpferei (f)	
raft	balsa (f)	radeau (m)	Floss (m)	
rattle	cascabel (m)	grelot (m)	Rassel (f), Schelle (f)	
reed	junco (m)	roseau (m)	Schilf (-rohr) (n)	
resin	resina (f)	résine (f)	Harz (m)	
ring of percussion	onde de percusión (f)	onde (f) de percussion	Schlagring (m), Schlagwelle (f)	
roof of the mouth	paladar (m)	palais (m)	Gaumen (m)	
s-twined	sinistrorso	tors gauche	linksgezwirnt	
sandstone	arenisca (f)	grès (m)	Sandstein (m)	
scalp (anat.)	cuero cabelludo	cuir chevelu	Kopfhaut (f), Haarboden (m)	

ENGLISH	SPANISH	FRENCH	GERMAN	ADDITIONAL LANGUAGES
scalp (ethn.)	cabellera desollada (f)	scalpe (m)	Skalp (m)
scraper	raspador (m)	racloir (m)	Schaber (m)
shed (text.)	tinglado (m)	pas (m)	Fach (n)
shell	concha (f)	coquille (f)	Muschel (f)
shellfish	molusco (m)	mollusque (m)	Muscheltier (n)
shellmound	conchal (m)	amas (m) coquillier	Muschelhügel (m)
shield, <i>see</i> buckler			
shinbone	tibia (f), canilla (f)	tibia (f)	Schienbein (n)
shoulderblade	omóplato (m)	omoplate (m)	Schulterblatt (n)
sieve	colador (m)	passoire (f)	Sieb (n)
sinew	tendón (m)	tendon (m)	Schne (f)
skull	cráneo (m)	crâne (m)	Schädel (m)
slate	pizarra (f)	ardoise (f)	Schiefer (m)
sling	honda (f)	fronde (f)	Schleuder (f)
slip (cer.)	engobe (m)	enduit (m)	Anguss (m), Überzug (m)
sodium nitrate	salitre (m)	salpêtre, nitrate (m)	Salpeter (m)
spear-thrower	estólica (f), propulsor (m)	propulseur (m)	Speer-, Pfeilschleuder (f)
spin	hilar	filer	spinnen
spine	columna vertebral (f)	colonne (f), vertebrale	Rückgrat (n), Wirbelsäule (f)
spindle	huso (m)	fuseau (m)	Spindel (f)
stature	estatura (f)	taille (f)	Wuchs (m), Körpergrösse (f)
stirrup handle	asa (f) en forma de estribo	anse (f) en forme d'étrier	(Steig-) Bügelhenkel (m)
strainer, <i>see</i> sieve			
striking platform	plataforma (f) de percusión	plan (m) de frappe	Schlagplattform (f)
superciliary arch	arco (m) superciliar	arc (m) superciliaire	Augenbrauenbogen (m)
temple	sien (f)	tempe (f)	Schläfe (f)
thigh	muslo (m)	cuisse (f)	Schenkel (m)
thigh bone	femur (m)	fémur (m)	Schenkelknochen (m)
thread	hilo (m)	fil (m)	Faden (m), Zwirn (m)
throat	gargante (f)	gorge (f)	Kehle (f)
tool	herramienta (f)	outil (m)	Werkzeug (n)
twill weave	ligatura (f) de sarga	armure (f) sergée	Körperbindung (f)
twist	torsión (f)	torsion (f)	Zwirnung (f), Drehung (f)
two-ply (text)	de dos cabos (m)	de deux brins (m)	zwei-elementig
upper jaw	maxilar (m)	maxillaire (m)	Oberkiefer (m)
vertebra	vertebra (f)	vertèbre (f)	Wirbelknochen (m), Vertebra (f)
waist	cintura (f)	ceinture (f)	Taille (f)
warp	urdimbre (f), cadena (f)	chaîne (f)	Kette (f)
wave of percussion, <i>see</i> ring of percussion			
weave	ligatura (f)	armure (f)	Bindung (f)
weft	trama (f)	trame (f)	Schuss (m)
wheel	rueda (f)	roue (f)	Rad (n)
whirl	fusaiola (f), tortera (f)	fusaiole (f)	Wirtel (m), Spinnblättchen (n)
wrist	muñeca (f)	poignet (m)	Rist (m), Handgelenk (n)
z-twined	dextrorso	tors (m) droit	rechtsgezwirnt

ESPAÑOL	INGLÉS	FRANÇÉS	ALEMÁN	ADDITIONAL LANGUAGES
acantilado (m)	cliff	falaise (f)	Kliff (n)
ajuar (m) funerario	grave goods	mobilier (m), funéraire	Grabbeigaben (f)
alfarería (f)	pottery	poterie (f)	Töpferei (f)
ámbar (m)	amber	ambre (m)	Bernstein (m)
ante-asta (m)	fore-piece	hampe (f)	Vorsatzstück (m), Vorscheft (m)
anzuelo (m)	fish-hook	hameçon (m)	Angel (f), Angelhaken (m)
arado (m)	plow	charrue (f)	Pflug (m)
arco (m) super-ciliar	superciliary arch	arc super-ciliaire	Augenbrauenbogen (m)
arenisca (f)	sandstone	grès (f)	Sandstein (m)
asa (f)	handle	anse (f)	Henkel (m)
asa en forma de estribo	stirrup-handle	anse en forme d'étrier	(Steig-) Bügelhenkel (m)
astilla (f)	flake	éclat (m)	Abschlag (m)
axilla (f)	axilla	aisselle (f)	Achsel (f)
azadón (m)	adze	bêche (f)	Haue (f), Hacke (f)
azagaya (f)	javelin	sagaie (f)	Speer (m)
balancín (m)	carrying pole	balancier (m)	Tragstange (f)

ESPAÑOL	INGLÉS	FRANÇÉS	ALEMÁN	ADDITIONAL LANGUAGES
balsa (f)	raft	radeau (m)	Floss (n)	
borde de menor a mayor	flaring rim	col évasé (m)	ausladender Rand (m)	
bulbo (m) de percusión	percussion bulb	bulbe (m), cone (m), de percusión	Schlagbuckel (m)	
brecha (geol.)	breccia	brèche (f)	Breccie, Brekzie (f)	
buril (m)	burin, graver, engraver's chisel	burin (m)	Stichel (m), Grabstichel (m)	
cabellera (f) desollada	scalp	scalpe (m)	Skalp (m)	
cabo (m) (text.)	ply	brin (m)	Element (n)	
cadena (f)	chain, warp (text.)	chaîne (f)	Kette (f)	
canasto (m)	basket	pannier (m)	Korb (m)	
cántaro (m)	pitcher	cruche (f)	Krug (m)	
cañamo (m)	hemp	chanvre (m)	Hanf (m)	
cara (f) de fisión	bulbar face	face (f) d'éclatement	Unterseite (f)	
cara (f) dorsal	dorsal face	face (f) dorsale	Oberseite (f)	
carenado	carinated	caréné	gekielt, Kiel-	
cascabel (m)	rattle	grelot (m)	Rassel (f), Schelle (f)	
celula (f)	cell	cellule (f)	Zelle (f)	
cepillo (m)	plane, brush	rabot (m), brosse (f)	Hobel (m), Bürste (f)	
cerbatana (f)	blowgun	sarbacane (f)	Blasrohr (n)	
cesteria (f)	basketry	vannerie (f)	Korbflechtere (f)	
cesteria en aduja	coiled basketry	roue (f) de cordage	Spiralwulstgeflecht (n)	
espiral				
cesto (m), véase canasto				
ciclo raso (m)	ceiling	plafond (m)	Decke (f), Plafond (m)	
cintura (f)	waist	taille (f)	Taille (f)	
cincel (m)	chisel	ciseau (m)	Meissel (m)	
cochura (f)	firing	cuisson (m)	Brennung (f)	
colador (m)	sieve, strainer	passoire (f)	Sieb (n)	
colapez (f)	fish-glue	colle (f) de poisson	Fischleim (m)	
columna (f) vertebral	spine	colonne (f) vertébrale	Wirbelsäule (f), Rückgrat (n)	
concha (f)	shell	coquille (f)	Muschel (f)	
conchal (m)	shellmound	amas (m) coquillier	Muschelhügel (m)	
copa (f)	beaker	coupe (f)	Becher (m)	
corteza (f)	bark	écorce (f)	Rinde (f)	
craneo (m)	skull	crâne (m)	Schädel (m)	
crisol (m)	crucible	creuset (m)	Schmelztiegel (m)	
cubo (m)	hub, nave	moyeu (m)	Nabe (f)	
cuero cabelludo (m)	scalp	cuir (m) chevelu	Kopfhaut (f)	
cuña (f) de mano	hand-axe	coup de poing (m)	Faustkeil (m)	
cuvada (f)	couvade	couvade (f)	Männerkindbett (n)	
de dos cabos (m)	two-ply	de deux brins (m)	zwei-elementig	
de un cabo (m)	one-ply	d'un brin	einelementig	
dentición (f)	dentition	dentition (f)	Zahnung (f)	
dextrorso	z-twined	tors (m) droit	rechtsgezwrnt	
dintel (m)	lintel	linteau (m)	(Tür-) Sturz (m)	
eje (m)	axle	axe (m)	obere Schwelle (f)	
empeine (m) del pie	instep	cou de pied (m)	Achse (f)	
engobe (m)	slip	enduit (m)	Spann (m), Rist (m) d. Fusses	
escondite (m)	cache	cache (f)	Anguss (m), Überzug (m)	
escudo (m)	buckler, shield, coat of arms	bouclier (m), écu (m)	Versteck (n)	
eslabón (m)	link	anneau (m)	Schild (m), Wappen (n)	
estaca (f) para cavar	digging-stick	bâton (m) de fouille	Glied (n)	
estatura (f)	stature	taille (f)	Grabstock (m)	
estolica (f)	spear-thrower	propulseur (m)	Wuchs (m), Körpergrösse (f)	
fémur (m)	thigh-bone, femur	fémur (m)	Speer-, Pfeilschleuder (f)	
flauta (f) de pan	pan-pipe	flûte (f) de Pan	Schenkelknochen (m), Femur (m)	
flecha (f)	arrow	flèche (f)	Panpfeife (f)	
formón (m)	chipping-tool	retouchoir (m)	Pfeil (m)	
fusaíola (f)	whorl	fusaíole (f)	Retouchierstab (m)	
garganta (f)	throat	gorge (f)	Wirtel (m), Spinnblättchen (n)	
gozne (m)	hinge	pivot (m)	Kehle (f)	
grano fino	fine grained	grain (m) fin	Angel (f) (archit.)	
grano (m) grueso	coarse grained	gros grain	feinkörnig	
grava (f)	gravel	gravier (m)	grobkörnig	
guijarro (m)	pebble	galet (m), caillou (m)	Schotter (m)	
hacha (f)	axe	hache (f)	Kiesel(-stein) (m), Knollen (m)	
hebra (f)	fiber	fibre (f)	Axt (f)	
herramienta (f)	tool	outil (m)	Faser (f), Fiber (f)	
hilar	spin	filer	Werkzeug (n)	
hilo (m)	thread	fil (m)	spinnen	
hoja (f)	blade, leaf	lame (f), feuille (f)	Faden (m), Zwirn (m)	
			Klinge (f), Blatt (n)	

ESPAÑOL	INGLÉS	FRANCS	ALEMAN	ADDITIONAL LANGUAGES
hombro (m)	shoulder	épaule (f)	Schulter (f)
honda (f)	sling	fronde (f)	Schleuder (f)
hulla (f)	bituminous coal	houille (f)	Steinkohle (f)
huso (m)	spindle	fuseau (m)	Spindel (f)
impresión (f)	finger print	empreinte (f)	Fingerabdruck (m)
digital		digitale	
juncos (m)	reed	roseau (m)	Schilf(-rohr) (n)
labio (m)	lip	lèvre (f)	Lippe (f)
lezna (f)	awl	poinçon (m)	Ahle (f)
ligatura (f)	weave	armure (f)	Bindung (text.) (f)
ligatura de cañamo, véase ligatura llana			
ligatura de sarga	twill weave	armure sergée	Köperbindung (f)
ligatura llana	plain weave	armure (f) toile	Leinenbindung (f)
línea (f) estriada	hatched line	ligne (f) striée	schräffierte Linie (f)
mandíbula (f)	lower jaw, mandible	mandibule (f)	Unterkiefer (m)
mango (m)	handle	manche (m)	Griff (m)
manopla (f)	knuckle-duster	coup de poing	Schlagring (m)
marfil (m)	ivory	ivoire (m)	Elfenbein (n)
maxilar (m)	upper jaw, maxillary	maxillaire (m)	Oberkiefer (m)
maza (m)	mace	massue (f), masse (f)	Keule (f)
microlito (m)	pigmy flint, microlith	microlith (m)	Zwergflint (m), Mikrolith (m)
mineral (m)	ore, mineral	minéral (m)	Erz, Mineral (n)
molusco (m)	shellfish	mollusque (m)	Muschel (f) (-tier) (n)
muñeca (f) (anat.)	wrist	poignet (m)	Rist (m), Handgelenk (n)
muslo (m)	thigh	cuisse (f)	Schenkel (m)
niña (f) del ojo	eye-ball	globe (m) de l'oeil	Augapfel (m)
nódulo (m)	core	nodule (m)	Kernstück (n)
núcleo (m)	nucleus	nucléus (m)	Nukleus (m)
omóplato (m)	shoulderblade	omoplate (m)	Schulterblatt (n)
onda (f) de percusión	ring, wave of percussion	onde (f) de percussion	Schlagring (m), -welle (f)
paladar (m)	roof of the mouth	palais (m)	Gaumen (m)
pantorrilla (f)	calf	mollet (m)	Wade (f)
párpado (m)	eyelid	paupière (f)	(Augen-)Lid (n)
partidor (m) (prehist.)	chopper	hâchoir (m)	Hackstein (m)
pecho (m)	chest	poitrine (f)	Brust (f), Brustkasten (m)
percutor (m)	hammerstone	percuteur (m)	Schlagstein (m)
pestaña (f)	eye lash	cil (m)	Wimper (f)
pico (m)	pick, beak	pic (m), bec (m)	Pickel (m), Schnabel (m)
picota (f)	pick-axe	pioche (f)	Pickel (m)
pedra (f) de tacitas	bedrock mortars	pierre (f) à cupules	Schalenstein (m)
pilar (m)	pillar	pilier (m)	Pfeiler (m)
pinza (f)	pincers	pince (f), pincette (f)	Pinzette (f)
piocha, véase picota			
piragua (f)	dugout	pirogue (f)	Finbaum (m)
pizarra (f)	slate	ardoise (f)	Schiefer (m)
plataforma (f) de percusión	striking platform	plan (m) de frappe	Schlagplattform (f)
pozo (m)	pit	puit (m)	Grube (f), Schacht (m)
propulsor, véase estólica			
punta de flecha	arrow-head	pointe (f) de flèche	Pfeilspitze (f)
punta (f) de lanza	lance head	pointe (f) à cran	Lanzenspitze
punta (f) en forma de hoja de laurel	laurel leaf point	feuille (f) de laurier	Lorbeerblattspitze (f)
punto (m) cruz	cross stitch	point (m) de croix	Kreuzstich (m)
punzón (m)	awl	poinçon (m)	Ahle (f)
puño (m)	fist	poing (m)	Faust (f)
quijada (f)	jaw	machoire (f)	Kiefer (m)
ranura (f)	groove	rainure (f)	Rille (f)
raspador (m)	grater, scraper, endscraper	grattoir (m), racloir (m)	Kratzer (m), Schaber (m)
raspador (m) terminal		grattoir sur bout de lame	Endkratzer (m), -schaber (m)
resina (f)	resin	résine (f)	Harz (n)
rodilla (f)	knee	genou (m)	Knie (n)
rompe-cabeza (m)	club	casse-tête (m)	Keule (f)
rueda (f)	wheel	roue (f)	Rad (n)
salitre (m)	sodium nitrate	salpêtre (m), nitrate (m)	Salpeter (m)
sedal (m)	fishing line	ligne (f)	Leine (f), Angelschnur (f)
sien (f)	temple	tempe (f)	Schläfe (f)
sinistrorso	s-twined	tors (m) gauche	linksgewirnt
taladro (m)	borer	perçoir (m) foret (m)	Bohrer (m)

ESPAÑOL	INGLÉS	FRANCÉS	ALEMÁN	ADDITIONAL LANGUAGES
taladro (m) de fuego	fire drill	tourniquet (m) de feu	Feuerbohrer (m), -quirl (m)
tapa (f)	lid	couvercle (m)	Deckel (m)
telar (m)	loom	métier (m)	Webstuhl (m)
tenazas (f)	pliers	tenaille (f)	Zange (f)
tendón (m)	sinew	tendon (m)	Schne (f)
tibia (f)	shin-bone	tibia (f)	Schienbein (n)
ringlado (m)	shed (text.)	pas (m)	Fach (n)
torno (m) de alfarero	potter's wheel	tour (m)	Töpferscheibe (f)
torsión (f)	twist	torsion (f)	Zwirnung (f), Drehung (f)
tortera (f), <i>véase</i> fusañola			
trama (f)	weft, woof, pick	trame (f)	Schuss (m)
urdimbre (f)	chain, warp	chaîne (f)	Kette (f)
vellón (m)	fleece	toison (m)	Vlies (n)
vertebra (f)	vertebra	vertèbre (f)	Wirbelknochen (m)
vidriado	glazed	vitriifié	glasiert
viga (f)	beam	poteau (m)	Balken (m)
yeso (m)	plaster	plâtre (m)	Gips (m), Bewurf (m)

FRANÇAIS	ANGLAIS	ESPAÑOL	ALLEMAND	ADDITIONAL LANGUAGES
aisselle (f)	axilla	axila (f)	Achsel (f)
amas (m) coquillier	shell-mound	conchal (m)	Muschelhügel (m)
ambre (m)	amber	ambar (m)	Bernstein (m)
anneau (m)	link	eslabón (m)	Glied (n)
anse (f)	handle	asa (f)	Henkel (m)
anse (f) en forme d'étrier	stirrup-handle	asa (f) en forma de estribo	(Steig-) Bügelheinkel (m)
arc (m) superciliaire	superciliary arch	arco (m) superciliar	Augenbrauenbogen (m)
ardoise (f)	slate	pizarra (f)	Schiefer (m)
armure (f)	weave	ligatura (f)	Bindung
armure (f) sergée	twill weave	ligatura (f) de sarga	Köperbindung (f)
armure (f) toile	plain weave	ligatura (f) llana l. de cáñamo	Leinenbindung (f)
axe (m)	axle	eje (m)	Achse (f)
balancier (m)	carrying pole	balancín (m)	Tragstange (f)
bâton (m) de fouille	digging-stick	estaca (f) para excavar	Grabstock (m)
bec (m)	beak	pico (m)	Schnabel (m)
hêche	adze	azadón (m)	Haue (f), Hacke (f)
bouclier (m)	buckler, shield	escudo (m)	Schild (m)
breche (f)	breccia	brecha (f)	Breccie (f), Brekzie (f)
brin (m)	ply	cabo (m)	Element (n)
bulbe (m) de percussion	percussion bulb	bulbo (m) de percusión	Schlagbuckel (m)
burin (m)	burin, graver	buril (m)	Stichel (m), Grabstichel (m)
cache (f)	cache	escondite (m)	Versteck (n)
caillou (m), <i>voir</i> galet			
carène	carinated	carenado	gekielt
casse-tête (m)	club	rompe-cabeza (m)	Keule (f)
cellule (f)	cell	celula (f)	Zelle (f)
chaîne (f)	chain, warp (text)	cadena (f) urdimbre (text)	Kette (f)
chanvre (m)	hemp	cañamo (m)	Hanf (m)
charrue (f)	plow	arado (m)	Pflug (m)
cil (m)	eye lash	pestaña (f)	Wimper (f)
ciseau (m)	chisel	cíncel (m)	Meißel (m)
col (m) évasé	flaring rim	borde de menor a mayor	ausladender Rand (m)
colle (f) de poisson	fish glue	colapez (f)	Fischleim (m)
colonne vertébrale	spine	columna (f) vertebral	Rückgrat (n), Wirbelsäule (f)
cone (m) de percus- sion, <i>voir</i> bulbe de percussion			
coquille (f)	shell	concha (f)	Muschel (f)
cou (m) de pied	instep	empcine (m) de pie	Spann (m), Rist (m)
coup (m) de poing	hand-axe knuckleduster	cuña de mano manopla (f)	Faustkeil (m) Schlagring (f)
coupe (f)	beaker	copa (f)	Becher (m)
couvade (f)	couvade	cuvada (f)	Männerkindbett (n)
couvercle (m)	lid	tapa (f)	Deckel (m)
crâne (m)	skull	cráneo (m)	Schädel (m)
creuset (m)	crucible	crisol (m)	Schmelztiegel (m)
cruche (f)	pitcher	cántaro (m)	Krug (m)
cuir (m) chevelu	scalp	cuero cabelludo	Kopfhaut (m)

FRANÇAIS	ANGLAIS	ESPAGNOL	ALLEMAND	ADDITIONAL LANGUAGES	
cuisse (f)	thigh	muslo (m)	Schenkel (m)		
cuisson (m)	firing	cochura (f)	Brennung (f)		
d'un brin (m)	one-ply	de un cabo (m)	einelementig		
de deux brins (m)	two-ply	de dos cabos (m)	zwei-elementig		
dentition (f)	dentition	dentición (f)	Zahnung (f)		
éclat (m)	flake	astilla (f)	Abschlag (m)		
écorce (f)	bark	corteza (f)	Rinde (f)		
écu (m)	coat of arms	escudo (m)	Wappen (n)		
empreinte (f)	finger print	impresión digital	Fingerabdruck (m)		
digitale					
enduit (m)	slip	engobe (m)	Anguss (m), Überzug (m)		
épaule	shoulder	hombro (m)	Schulter (f)		
face (f) d'éclatement	bulbar face	cara (f) de fisión	Unterseite (f)		
face dorsale	dorsal face	cara (f) dorsal	Oberseite (f)		
falaise (f)	cliff	acantilado (m)	Kliff (n)		
fémur (m)	thigh-bone	femur (m)	Schenkelknochen (m), Femur (m)		
feuille (f) de laurier	laurel leaf point	punta en forma de hoja de laurel	Lorbeerblattspitze (f)		
fibre (f)	fiber	hebra (f)	Faser (f), Fiber (f)		
fil (m)	thread	hilo (m)	Faden (m), Zwirn (m)		
filer	spin	hilar	spinnen		
flèche (f)	arrow	flecha (f)	Pfeil (m)		
flûte (f) de Pan	Pan-pipe	flauta (f) de Pan	Panpfeife (f)		
foret (m)	borer	taladro (m)	Bohrer (m)		
fronde (f)	sling	honda (f)	Schleuder (f)		
fusaïole (f)	whirl	fusaïola (f) tortera (f)	Wirtel (m) Spinnblättchen (n)		
fuseau (m)	spindle	huso (m)	Spindel (f)		
galet (m)	pebble	guijarro (m)	Kiesel (-stein) (m), Knollen (m)		
genou (m)	knee	rodilla (f)	Knie (n)		
globe (m) de l'oeil	cye-ball	niña (f) del ojo	Augapfel (m)		
gorge (f)	throat	garganta (f)	Kehle (f)		
grain (m) fin	fine-grained	grano (m) fino	feinkörnig		
gros grain (m)	coarse-grained	grano grueso	grobkörnig		
grattoir (m)	grater	raspador (m)	Kratzer (m)		
grattoir (m) sur bout de lame	endscraper	raspador (m) terminal	Endkratzer (m), Endschaber (m)		
gravier (m)	gravel	grava (f)	Schotter (m)		
grelot (m)	rattle	cascabel (m)	Rassel (f), Schelle (f)		
grès (m)	sandstone	arenisca (f)	Sandstein (m)		
hache (f)	axe	hacha (f)	Axt (f)		
hâchoir (m) (prehist.)	chopper	partidor (m)	Hackstein (m)		
hameçon (m)	fish-hook	anzuelo (m)	Angel (f), Angelhaken (m)		
hampe (f)	fore-piece	ante-asta (f)	Vorsatzstück (n) Vorschaft (m)		
houille (f)	bituminous coal	hulla (f)	Steinkohle (f)		
ivoire (m)	ivory	marfil (m)	Elfenbein (n)		
lame (f)	blade	hoja (f)	Klinge (f)		
lèvre (f)	lip	labio (m)	Lippe (f)		
ligne (f)	fishing-line	sedal (m)	Leine (f), Angelschnur (f)		
ligne (f) striée	hatched line	linea (f) estriada	schrattierte Linie (f)		
linteau (m)	lintel	dintel (m)	(Tür-) Sturz (m) obere Schwelle (f)		
mâchoire (f)	jaw	quijada (f)	Kiefer (m)		
mandibule (f)	lower jaw, mandible	mandíbula (f)	Unterkiefer (m)		
manche (f)	handle	mango (m)	Griff (m)		
masse (f), voir massue					
massue (f)	mace	maza (f)	Keule (f)		
maxillaire (m)	upper jaw	maxilar (m)	Oberkiefer (m)		
métier (m)	loom	telar (m)	Webstuhl (m)		
microlith (m)	microlith, pigmy flint	microlite (m)	Mikrolith (m), Zwergflint (m)		
minéral (m)	ore	mineral (m)	Erz (n), Mineral (n)		
mobilier (m)	grave-goods	ajuar funerario	Grabbeigaben (f)		
funéraire					
mollet (m)	calf	pantorilla (f)	Wade (f)		
mollusque (m)	shellfish	molusco (m)	Muschel (f), Muscheltier (n)		
moyeu (m)	hub, nave	cuho (m)	Nabe (f)		
nitrate (m), voir salpêtre					
nodule (m)	core	nódulo (m)	Kernstück (n)		
nucléus (m)	nucleus	núcleo (m)	Nukleus (m)		
omoplate (m)	shoulderblade	omóplate (m)	Schulterblatt (n)		
onde (f) de percussion	ring of percussion	onda (f) de percusión	Schlagring (m), Schlagwelle (f)		
orbite (f)	eye socket	órbita	Augenhöhle (f)		
outil (m)	tool	herramienta (f)	Werkzeug (n)		
palais (m)	roof of the mouth	paladar (m)	Gaumen (m)		

FRANÇAIS	ANGLAIS	ESPAGNOL	ALLEMAND	ADDITIONAL LANGUAGES
pannier (m)	basket	canasto (m), cesto (m)	Korb (m)	
pas (m) (text.)	shed	tinglado (m)	Fach (n)	
passoire (f)	sieve, strainer	colador (m)	Sieb (n)	
paupière	eyelid	parpado (m)	(Augen-) lid (n)	
perçoir, voir foret				
percuteur (m)	hammerstone	percutor (m)	Schlagstein (m)	
pic (m)	pick	pico (m)	Pickel (m)	
pièce (f) à cupules	bedrock mortars	pedra (f) de tacitas	Schalenstein (m)	
pilier (m)	pillar	pilar (m)	Pfeiler (m)	
pince (f)	pincers	pinza (m)	Pinzette (f)	
pincette (f), voir pince				
pioche (f)	pick-axe	piocha (f), picota (f)	Pickel (m)	
pirogue (f)	dugout	piragua (f)	Einbaum (m)	
pivot (m)	hinge	gozne (m)	Angel (f)	
plafond (m)	ceiling	ciclo raso (m)	Decke (f), Plafond (m)	
plan (m) de frappe	striking platform	plataforma (f) de percusión	Schlagplattform	
plâtre (m)	plaster	yaso (m)	Gips (m), Bewurf (m)	
poignet (m)	wrist	muñeca (f)	Rist (m), Handgelenk (n)	
poing (m)	fist	puño (m)	Faust (f)	
poinçon (m)	awl	punzón (m), lezna (m)	Ahle (f)	
point (m) de croix	cross stitch	punto cruz (m)	Kreuzstich (m)	
pointe (f) à cran	lance head	punta (f) de lanza	Lanzenspitze (f)	
pointe (f) de flèche	arrow head	punta (f) de flecha	Pfeilspitze (f)	
poitrine (f)	chest	pecho (m)	Brust (f), -kasten (m)	
poteau (m)	beam	viga (f)	Balken (m)	
poterie (f)	pottery	alfarería (f)	Töpferei (f)	
propulseur (m)	spear thrower	estólica (f)	Speer-, Pfeil-	
		propulsor (m)	schleuder (f)	
puit (m)	pit	pozo (m)	Grube (f), Schacht (m)	
rabot (m)	plane	cepillo (m)	Hobel (m)	
racloir (m)	scraper	raspador (m)	Schaber (m)	
radeau (m)	raft	balsa (f)	Floss (n)	
rainure (f)	groove	ranura (f)	Rille (f)	
résine (f)	resin	resina (f)	Harz (n)	
retouchoir (m)	chipping tool	formón (m)	Retouchierstab (m)	
roseau (m)	reed	junco (m)	Schilf (n), -rohr (n)	
roue (f)	wheel	rueda (f)	Rad (n)	
roue (f) de cordage	coiled basketry	cestería (f) en aduja	Spiralwulst-	
		espiral	geflecht (n)	
sagaie (f)	javelin	azagaya (f)	Speer (m)	
salpêtre (m)	sodium nitrate	salitre (m)	Salpeter (m)	
sarbacane (f)	blow-gun	cerbatana (f)	Blasrohr (n)	
scalpe (m)	scalp	cabellera (f)	Skalp (m)	
		desollada		
taille (f)	stature, waist	estatura (f), cintura (f)	Körpergröße (f), Wuchs (m)	
tempe (f)	temple	sien (f)	Schläfe (f)	
tenaille (f)	pliers	tenazas (f)	Zange (f)	
tendon (m)	sinew	tendón (m)	Sehne (f)	
tibia (f)	shin-bone	tibia (f)	Schienbein (n)	
toison (m)	fleece	vellón (m)	Vlies (n)	
tors (m) droit	z-twined	dextrorso	rechtsgezirnt	
tors (m) gauche	s-twined	sinistrorso	linksgezirnt	
torsion (f)	twist	torsión (f)	Zwirnung (f), Drehung (f)	
tour (m)	potter's wheel	torno de alfarero (m)	Töpferscheibe (f)	
tourniquet (m)	fire drill	taladro (m) de fuego	Feuerbohrer (m), -quirl (m)	
trame (f)	weft, woof, pick	trama (f)	Schuss (m)	
vannerie (f)	basketry	cestería (f)	Korbflechterei (f)	
vertèbre (f)	vertebra	vertebra (f)	Wirbelknochen (m)	
vitriifié	glazed	vidriado	glasiert	
DEUTSCH	ENGLISCH	SPANISCH	FRANZÖSISCH	ADDITIONAL LANGUAGES
Abschlag (m)	flake	astilla (f)	éclat (m)	
Achse (f)	axle	eje (m)	axe (m)	
Achsel (f)	axilla	axilla (f)	aisselle (f)	
Ahle (f)	awl	punzón (m), lezna (f)	poinçon (m)	
Angel (f) (archit.)	hinge	gozne (m)	pivot (m)	
Angel (f) (-haken) (m)	fish-hook	anzuelo (m)	hameçon (m)	
Angelschnur (f)	fishingline	sedal (m)	ligne (f)	
Anguss (m)	slip	engobe (m)	enduit (m)	
Augapfel (m)	eye-ball	niña del ojo	globe (m) de l'ocil	
Augenbrauenbogen (m)	superciliary arch	arco (m) superciliar	arc (m) superciliaire	
Augenhöhle (f)	eye socket	orbita (f)	orbite (f)	
Augenlid, siehe Lid				

DEUTSCH	ENGLISCH	SPANISCH	FRANZÖSISCH	ADDITIONAL LANGUAGES	
ausladender	flaring rim	borde de menor	col (m) évasé
Rand (m)		a mayor	
Axt (f)	axe	hacha (f)	hache (f)
Balken (m)	beam	viga (f)	poteau (m)
Becher (m)	beaker	copa (f)	coupe (f)
Bernstein (m)	amber	ambar (m)	ambre (m)
Bewurf (archit.)	plaster	enyesado (m)	plâtre (m)
Bindung (f) (text.)	weave	ligatura (f)	armure (f)
Blasrohr (n)	blow-gun	cerbatana (f)	sarbacane (f)
Bohrer (m)	borer	taladro (n)	foret (m), perçoir (m)
Breccie, Breckzie (f)	breccia	brecha (f)	brèche (f)
Brennung (f)	firing	cochura (f)	cuisson (m)
Brust (f) (-kasten, m)	chest	pecho (m)	poitrine (f)
Bügelhenkel, siehe Steigbügelhenkel			
Decke (f) (archit.)	ceiling	cielo raso (m)	plafond (m)
Deckel (m)	lid	tapa (f)	couvercle (m)
Drehung (f) (text.)	twist	torsión (f)	torsion (f)
Einbaum (m)	dugout	piragua (f)	pirogue (f)
einementig (text.)	one-ply	de un cabo (m)	d'un brin (m)
Element (text.)	ply	cabo (m)	brin (m)
Elfenbein (n)	ivory	marfil (m)	ivoire (m)
Endschaber, -kratzer (m)	endscraper	raspador (m)	grattoir (m) sur bout de lame
Erz, siehe Mineral		terminal	
Fach (n) (text.)	shed	tinglado (m)	pas (m)
Faden, siehe Zwirn			
Faser (f)	fiber	hebra (f)	fibre (f)
Faust (f)	fist	puño (m)	poignet (m)
Faustkeil (m)	hand-axe	cuña (f) de mano	coup (m) de poing
feinkörnig	fine-grained	grano (m) fino	grain (m) fin
Femur, siehe Schenkelknochen			
Feuerbohrer, -quirl (m)	fire drill	taladro (m) de fuego	tourniquet (m) de feu
Fiber, siehe Faser			
Fingerabdruck (m)	fingerprint	impresión digital	empreinte (f) digitale
Fischleim (m)	fishglue	colapez (f)	colle (f) de poisson
Floss (n)	raft	balsa (f)	radeau (m)
Gaumen (m)	roof of the mouth	paladar (m)	palais (m)
gekielt	carinated	carenado	caréné
Glied (n)	link, member, limb	eslabón (m)	anneau, membre
		miembre	
Gips (m)	plaster	veso	plâtre (m)
glasiert	glazed	vidriado	vitrifié
Grabbeigaben (f)	grave-goods	ajuar funerario	mobilier (m) funéraire
Grabstichel, siehe Stichel			
Grabstock (m)	digging-stick	estaca (f) para excavar	bâton (m) de fouille
Griff (m)	handle	mango (m)	manche (m)
grobkörnig	coarse-grained	grano grueso	gros grain (m)
Grube, siehe Schacht			
Hacke (f)	adze	azadón (m)	bèche (f)
Hackstein (m)	chopper (prehist.)	partidor (m)	hâchoir (préhist.)
Handgelenk (n)	wrist	muñeca (f) anat.)	poignet (m)
Hanf (m)	hemp	cañamo (m)	chanvre (m)
Harz (n)	resin	resina (f)	résine (f)
Haut, siehe Hacke			
Henkel (m)	handle	asa (f)	anse (f)
Hobel (m)	plane	cepillo (m)	rabot (m)
Kehle (f)	throat	garganta (f)	gorge (f)
Kernstück (n)	core	nódulo (m)	nodule (m)
Kette (f)	chain	cadena (f), urdimbre (f) (text.)	chaîne (f)
Keule (f)	club, mace	rompe-cabeza (m), maza (f)	casse-tête (m), massue, masse (f)
Kiefer (m)	jaw	quijada (f)	mâchoire (f)
Kiesel (-stein) (m)	pebble	guijarro (m)	galet (m), caillou (m)
Kliff (n)	cliff	acantilado (m)	falaise (f)
Klinge (f)	blade	hoja (f)	lame (f)
Knie (n)	knee	rodilla (f)	genou (m)
Knollen, siehe Kiesel			
Körperbindung (f)	twill weave	ligatura (f) de sarga	armure (f) sergée
Körpergrösse (f)	stature	estatura (f)	taille (f)
Kopfhaut (f)	scalp	cuero cabelludo	cuir (m) chevelu
Korb (m)	basket	canasto (m), cesto (m)	pannier (m)
Korbflechtere (f)	basketry	cestería (f)	vannerie (f)
Kratzer (m)	grater	raspador (m)	grattoir (m)
Kreuzstich (m)	cross-stitch	punto (m) cruz	point (m) de croix
Krug (m)	pitcher	cántaro (m)	cruche (f)
Lanzenspitze (f)	lance head	punta (f) de lanza	pointe (f) à cran
Leine, siehe Angelschnur			

DEUTSCH	ENGLISCH	SPANISCH	FRANZÖSISCH	ADDITIONAL LANGUAGES
Leinenbindung (f)	plain weave	ligatura (f) llana, ligatura de cáñamo	armure (f) toile
Lid (n)	eyelid	párpado (m)	paupière (f)
linksgezirnt	s-twined	sinistrorso	tors (m) gauche
Lippe (f)	lip	labio (m)	lèvre (f)
Lorbeerblattspitze (f)	laurel leaf point	punta (f) en forma de laurel	feuille (f) de laurier
Männerkindbett (n)	couvade	cuvada (f)	couvade (f)
Meissel (m)	chisel	cincel (m)	ciseau (m)
Mineral (n)	ore	mineral (m)	minéral (m)
Muschel (f)	shell	concha (f)	coquille (f)
Muscheltier (n)	shellfish	molusco (m)	mollusque (m)
Muschelhügel (m)	shellmound	conchal (m)	amas (m) coquillier
Nabe (f)	hub, nave	cubo (m)	moyeu (m)
Nukleus (m)	nucleus	nucleo (m)	nucléus (m)
Oberkiefer (m)	upper jaw	maxilar (m)	maxillaire (m)
Oberschwelle (f)	lintel	dintel (m)	linteau (m)
Oberseite (f) (prehist.)	dorsal face	cara (f) dorsal	face (f) dorsale
Panpfeife (f)	Pan pipe	flauta (f) de Pan	flûte (f) de Pan
Pfeil (m)	arrow	flecha (f)	flèche (f)
Pfeiler (m)	pillar	pilar (m)	pilier (m)
Pfeilschleuder (f)	spear-thrower	estólica (f), propulsor (m)	propulseur (m)
Pfeilspitze (f)	arrow-head	punta (f) de flecha	pointe (f) de flèche
Pflug (m)	plow	arado (m)	charrue (f)
Plafond (m), siehe Decke			
Pickel (m)	pick, pick-axe	pico (m), picota (f), piocha (f)	pioche (f)
Pinzette (f)	pincers	pinza (f)	pince (f), pincette
Rad (n)	wheel	rueda (f)	roue (f)
Rassel (f)	rattle	cascabel (m)	grelot (m)
rechtsgezirnt	z-twined	dextrorso	tors (m) droit
Retouchierstab (m)	chipping-tool	formón (m)	retouchoir (m)
Rille (f)	groove	ranura (f)	rainure (f)
Rinde (f)	bark	corteza (f)	écorce (f)
Rist (m), siehe Handgelenk, Spann			
Rückgrat (n)	spine	columna (f) vertebral	colonne (f) vertébrale
Salpeter (m)	sodium nitrate	salitre (m)	salpêtre (m), nitrate (m)
Sandstein (m)	sandstone	arenisca (f)	grès (m)
Schaber (m)	scraper	raspador (m)	racloir (m)
Schacht (m)	pit	pozo (m)	puits (m)
Schädel (m)	skull	cráneo (m)	crâne (m)
Schalenstein (m)	bedrock mortars	piedra (f) de tacitas	pietre (f) à cupules
Schelle (f), siehe Rassel			
Schenkel (m)	thigh	muslo (m)	cuisse (f)
Schenkelknochen	thigh-bone	femur (m)	fémur (m)
Schiefel (m)	slate	pizarra (f)	ardoise (f)
Schienbein (n)	shinbone	tibia (f)	tibia (f)
Schild (m)	shield, buckler	escudo (m)	bouclier (m)
Schilf (-rohr) (n)	reed	juncos (m)	roseau (m)
Schläfe (f)	temple	sien (f)	tempe (f)
Schlagbuckel (m)	percussion bulb	bulbo (m) de percusión	bulbe (m) (conc) de percussion
Schlagplattform (f)	striking platform	plataforma (f) de percusión	plan (m) de frappe
Schlagring (m), -welle (f)	ring of percussion	onda (f) de percusión	onde (f) de percussion
Schlagring (m)	knuckleduster	manopla (f)	coup (m) de poing
Schlagstein (m)	hammerstone	percutor (m)	percuteur (m)
Schleuder (f)	sling	honda (f)	fronde (f)
Schmelztiegel (m)	crucible	crisol (m)	creuset (m)
Schnabel (m)	beak	pico (m)	bec (m)
Schotter (m)	gravel	grava (f)	gravier (m)
Schraffierte Linie (f)	hatched line	línea (f) estriada	ligne (f) striée
Shulter (f)	shoulder	hombro (m)	épaule (f)
Schulterblatt (n)	shoulderblade	omóplate (m)	omoplate (m)
Schuss (m) (text.)	weft, woof, pick	trama (f)	trame (f)
Schne (f)	sinew	tendón (m)	tendon (m)
Sieb (n)	sieve, strainer	colador (m)	passoire (f)
Skalp (m)	scalp	cabellera (f) desollada	scalpe (m)
Spann (m)	instep	empeine del pied	cou (m) de pied
Speer (m)	javelin	azagaya (f)	sagaie (f)
Pfeilschleuder (f), siehe Speerschleuder			
Spindel (f)	spindle	huso (m)	fuseau (m)

DEUTSCH	ENGLISCH	SPANISCH	FRANZÖSISCH	ADDITIONAL LANGUAGES
Spinnblättchen (n), siehe Wirtel				
spinnen	spin	hilar	filer	
Spiralwulstgeflecht (n)	coiled basketry	cestería (f) en aduja espiral	roue (f) de cordage	
Steigbügelhenkel (m)	stirrup-handle	asa en forma de estribo	anse (f) en forme d'étrier	
Steinkohle (f)	bituminous coal	hulla (f)	houille (f)	
Stichel (m)	burin, graver	buril (m)	burin (m)	
Taille (f)	waist	cintura (f)	taille (f)	
Tibia (f), siehe Schienbein				
Töpferei (f)	pottery	alfarería (f)	poterie (f)	
Töpferscheibe (f)	potter's wheel	torno (m) de alfarero	tour (m)	
Tragstange (f)	carrying pole	balancín (m)	balancier (m)	
Türsturz (m), siehe Oberschwelle				
Überzug (m), siehe Anguss				
Unterkiefer (m)	lower jaw	mandíbula (f)	mandibule (f)	
Unterseite (f) (prehist.)	bulbar face	cara (f) de fisión	face (f) d'éclatement	
Versteck (n)	cache	escondite (m)	cache (f)	
Vlies (n)	fleece	vellón (m)	toison (m)	
Vorsatzstück (n), siehe Vorscheft				
Vorscheft (m)	fore-piece	ante-asta (f)	hampe (f)	
Wade (f)	calf	pantorilla (f)	mollet (m)	
Wappen (n)	coat of arms	escudo (m)	écu (m)	
Webstuhl (m)	loom	telar (m)	métier (m)	
Werkzeug (n)	tool	herramienta (f)	outil (m)	
Wimper (f)	eye-lash	pestaña (f)	cil (m)	
Wirbelknochen (m)	vertebra	vertebra (f)	vertèbre (f)	
Wirbelsäule (f), siehe Rückgrat				
Wirtel (m)	whirl	fusaiola (f), tortera (f)	fusaiole (f)	
Zahnung (f)	dentition	dentición (f)	dentition (f)	
Zange (f)	pliers	tenazas (f)	tenaille (f)	
Zelle (f)	cell	célula (f)	cellule (f)	
zwei-elementig (text)	two-ply	de dos cabos (m)	de deux brins (m)	
Zwergflint	pigmy flint	microlito (m)	microlith (m)	
Zwirn (m)	thread	hilo (m)	fil (m)	
Zwirnung (f), siehe Drehung				

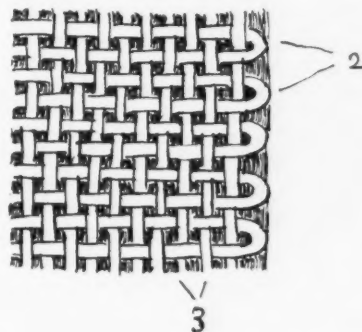
PROPOSED TOPICAL OUTLINE

1. Raw Materials
 - 1.1 Inorganic
 - 1.11 stone
 - 1.12 metal
 - 1.13 clay
 - 1.2 Organic
 - 1.21 shell
 - 1.22 wood
 - 1.23 bone
 - 1.24 leather, hide
 - 1.25 wool
 - 1.26 vegetal material
 - 1.27 fat
 - 1.28 resin
 2. Geographical and Geological Terms
 - 2.1 Water
 - 2.11 fresh water
 - 2.12 sea water
 - 2.2 Earth
 - 2.21 geological formations
 - 2.22 geographical features
 - 2.3 Climate
 3. Fauna
 - 3.1 Mammals
 - 3.11 parts of mammals
 - 3.2 Birds
 - 3.21 parts of birds
 - 3.3 Fishes and Mollusks
 - 3.31 parts of fishes and mollusks
 - 3.4 Insects
 - 3.41 parts of insects
 - 3.5 Reptiles
 - 3.51 parts of reptiles
 4. Flora
 - 4.1 Types of Vegetation
 - 4.2 Cultivated plants
 - 4.21 cereals
 - 4.22 fruits
 - 4.23 vegetables, etc.
 - 4.3 Utilized Plants
 - 4.4 Parts of Plants
 5. Physical Anthropology
 - 5.1 Human Body
 - 5.11 skeleton
 - 5.12 fleshy parts
 - 5.13 deformations
 - 5.14 pathology
 - 5.2 Racial Types
 - 5.21 fossil
 - 5.22 modern
 6. Tools
 - 6.1 Technique of stone work
 - 6.11 nucleus
 - 6.12 flakes
 - 6.2 Prehistoric industries
 - 6.21 names
 - 6.3 Prehistoric tools
 - 6.31 burins
 - 6.32 scrapers
 - 6.33 knives
 - 6.34 saws
 - 6.35 points
 - 6.36 perforators
 - 6.37 axes
 - 6.38 adzes
 - 6.39 fish-hooks
 - 6.40 harpoons
 - 6.4 Fire
 - 6.41 fire-making
 - 6.5 Actions and qualities
 - 6.6 Cutting tools
 - 6.7 Perforating tools
 - 6.8 Grinding tools
 - 6.9 Percussion tools
 - Etc. (Classification of this chapter is not definite)
 7. Weapons
 - 7.1 Hand weapons
 - 7.11 lances
 - 7.12 swords
 - 7.13 daggers
 - 7.2 Throwing weapons
 - 7.21 spears
 - 7.22 spear-throwers
 - 7.23 boomerangs and throwing-knives
 - 7.24 arrows
 - 7.25 bows
 - 7.251 accessories
 - 7.26 projectile points
 - 7.261 parts
 - 7.3 Protective weapons
 - 7.31 helmets
 - 7.32 cuirasses
 - 7.33 shields
 - 7.331 parts
 8. Architecture
 - 8.1 Settlement patterns
 - 8.2 Types of habitations
 - 8.21 parts of habitations
 - 8.211 foundations
 - 8.212 walls
 - 8.213 doors and windows
 - 8.2131 parts
 - 8.214 columns and pillars
 - 8.2141 parts
 - 8.215 roofs
 - 8.2151 parts of roofs
 - 8.216 arches
 - 8.217 stairs and corridors
 - 8.218 terraces, balconies
 - 8.219 floors
 - 8.220 rooms
 - 8.2201 parts
 - 8.3 Materials of construction (*see also* chap. 1)
 - 8.31 stone
 - 8.32 wood, straw, etc.
 - 8.33 bricks, clays, cements
 - 8.4 Furniture and household items
 - 8.5 Hydraulic constructions
 - 8.6 Funerary constructions (*see also* chap. 22)
 - 8.7 Other constructions
9. Hunting
 - 9.1 Methods
 - 9.2 Weapons (*see also* chaps. 6, 7)
 - 9.3 Traps
10. Fishing
 - 10.1 Methods
 - 10.2 Weapons (*see also* chap. 6)
 - 10.3 Poisons
 - 10.4 Nets
 - 10.5 Other accessories
11. Agriculture
 - 11.1 Tools (*see also* chap. 6)
 - 11.2 Types of cultivation (*see also* chap. 4)
 - 11.3 Irrigation (*see also* chap. 8)
 - 11.4 Preservation and storage
12. Animal Husbandry
 - 12.1 Animals (*see also* chap. 3)
 - 12.2 Pasturing
 - 12.3 Stables (*see also* chap. 8)
13. Transport
 - 13.1 Land transport
 - 13.11 roads
 - 13.12 bridges
 - 13.13 vehicles
 - 13.131 carts

- 13.1311 parts
- 13.132 sledges
 - 13.1321 parts
- 13.133 other means
- 13.14 animal traction
 - 13.141 harnesses, etc.
- 13.15 human transport
 - 13.151 accessories
- 13.2 Water transport
 - 13.21 types of boats
 - 13.211 parts of boats
- 14. Consumption
 - 14.1 Food
 - 14.11 solid
 - 14.12 liquid
 - 14.2 Preservation (*see also* chaps. 3, 4, 9, 10, 11, 12)
- 15. Pottery
 - 15.1 Raw materials (*see also* chap. 1)
 - 15.2 Kilns and baking
 - 15.3 Tools
 - 15.4 Techniques
 - 15.5 Shapes
 - 15.51 parts of shapes
 - 15.6 Decorations (*see also* chap. 20)
- 16. Weaving
 - 16.1 Raw materials (*see also* chap. 1)
 - 16.2 Dyes
 - 16.21 dyeing techniques
 - 16.3 Spinning
 - 16.31 accessories
 - 16.4 Looms
 - 16.41 parts of looms
 - 16.5 Techniques
 - 16.51 types of cloth
 - 16.52 parts of cloth
 - 16.53 sewing and embroidery
 - 16.531 tools
 - 16.54 basketry
 - 16.541 techniques
- 16.55 matting and braiding
 - 16.551 techniques
- 16.56 knots, netting
 - 16.561 techniques
- 16.57 tools
- 17. Metallurgy
 - 17.1 Raw materials (*see also* chap. 1)
 - 17.2 Tools and implements (*see also* chap. 6)
 - 17.3 Furnaces and their parts
 - 17.4 Techniques
- 18. Dress and Ornament
 - 18.1 Dress
 - 18.11 head
 - 18.12 chest
 - 18.13 hips
 - 18.14 legs and feet
 - 18.15 accessories
 - 18.16 materials
 - 18.2 Ornaments
 - 18.21 on body
 - 18.211 painting
 - 18.212 tattooing
 - 18.22 on dress (*see also* chap. 16)
 - 18.23 independent (jewelry)
 - 18.3 Care of body
 - 18.4 Hair-dressing
 - 18.5 Accessories and implements
- 19. Social Organization
 - 19.1 Organization of the family
 - 19.11 marriage
 - 19.12 kinship terms
 - 19.2 Organization of the community
 - 19.21 societies
 - 19.22 initiation
 - 19.23 festivals
 - 19.3 Economic organization
- 19.4 Political organization
- 19.5 Law and transgressions
- 19.6 Warfare
- 20. Arts
 - 20.1 Sculpture
 - 20.2 Engraving
 - 20.3 Painting
 - 20.31 elements of decoration (*see also* chap. 15)
 - 20.4 Music
 - 20.41 musical instruments and parts
 - 20.42 songs
 - 20.5 Dancing
 - 20.6 Drama
 - 20.7 Literature
- 21. Cult
 - 21.1 Religion
 - 21.2 Magic
 - 21.3 Priesthood and equivalents
 - 21.4 Symbols and rituals
 - 21.5 Sites of cults
 - 21.6 Objects of cults
- 22. Sepulture
 - 22.1 Tombs (*see also* chap. 8)
 - 22.2 Preparation of body
 - 22.3 Funerary trousseau
 - 22.4 Funerary rites (*see also* chap. 21)
- 23. Science
 - 23.1 Writing
 - 23.2 Measures
 - 23.3 Astronomy, calendar
 - 23.4 Medicine
 - Etc.
- 24. Abstract Terms
 - 24.1 Linguistics
 - 24.2 Methods and theories
 - Etc.

SAMPLE OF ENTRY IN OUTLINE

- 1. plain weave
 - ligatura (f) llana,—de cáñamo
 - armure (f) toile (f)
 - Leinenbindung (f)
- 2. weft, woof, pick
 - trama (f)
 - trame (f)
 - Schuss (-faden) (m)
- 3. warp, chain
 - urdimbre (f), cadena (f)
 - chaîne (f)
 - Kette (f), Kettenfaden (m)



1960 Viking Fund Medalists

The 1960 Viking Fund Medals and Awards for outstanding achievement were presented on March 3, 1961, to three well-known scientists: Samuel K. Lothrop, Peabody Museum, Cambridge, Mass., U.S.A. (archaeology); Leslie Spier, Santa Cruz, California, U.S.A. (general anthropology); and Sherwood L. Washburn, University of California, Berkeley, California, U.S.A. (physical anthropology). These awards mark the 15th and final presentations made to scientists annually selected by the American Anthropological Association, the American Association of Physical Anthropologists, and the Society for American Archaeology (ca May 1960, p. 257). Dr. Paul Fejos, President of the Wenner-Gren Foundation, which gives the awards, announced in 1959 that after 1960 a single award will be presented on a world-wide basis. The Foundation has now formally invited Associates in CA to choose the 1961 Viking Fund Medalist.

Personnel Wanted

... A physical anthropologist, well acquainted with anthropometric methods as well as with genetic and anthropological concepts, to do field work among Brazilian Indians and give an advanced physical anthropology course at the University of Rio Grande do Sul, Brazil. The University, which is conducting a long-term study of Indians of Brazil, particularly from the viewpoint of human population genetics (demography, certain normal morphological characteristics of the discontinuous type, blood groups, types of hemoglobin and haptoglobin), needs an anthropologist to study these populations with the methods of classical anthropometry, providing comparative data for the anthropometric and genetic approaches. The position would be for one year, starting about July, 1961. Letters, with curricula vitae and additional details, should be sent to FRANCISCO M. SALZANO, Ph.D., Priv. Doc., Instituto de Ciências Naturais, Av. Paulo Gama, Porto Alegre, Rio Grande do Sul.

Wanted

► References to publications issued between 1947 and 1959, particularly in local, regional, and foreign journals, which deal with the archeology, ethnology, and ethnohistory of the eastern

United States and Canada. This area includes the states and provinces bordering on the Atlantic Ocean, as well as Ontario, Quebec, Michigan, and Alabama. The references are wanted for *An Anthropological Bibliography of the Eastern Seaboard, Volume II*, which the University of Oklahoma Research Institute is preparing with the aid of a grant from the (U.S.) National Science Foundation. The *Bibliography* will be published by the Eastern States Archeological Federation as Research Publication Number 2. References should be sent to the Editor, ALFRED K. GUTHE, Rochester Museum of Arts and Sciences, 657 East Avenue, Rochester 7, New York, U.S.A.

► . . . Reprints of ethnomusicological publications on the music of the world, with special emphasis on Asia. Please send to, and bill, the MUSIC LIBRARY, UNIVERSITY OF THE PHILIPPINES, Diliman, Rizal, Philippines.

► . . . Offprints of anthropological publications, by the Anthropology Centre of the University of Western Australia, which is forming a departmental Offprint Library. The Centre, established in 1956, has found it difficult to obtain out-of-print books, as well as back numbers of anthropological and sociological journals. It is particularly anxious to obtain books on Aboriginal Australia, the Pacific, Southeast Asia, and India, as well as those of a general anthropological nature. Since the Library's funds are limited, duplicate copies of current books would be very welcome; and if desired, staff members of the Centre are very willing to give copies of their own publications in return.—R. M. BERNDT, Anthropology Centre, University of Western Australia, Nedlands, Western Australia.

► Information about slabs with rock engravings from Griqualand West (Northern Cape) housed in overseas museums would be most gratefully received.—GERHARD J. FOCK, Archaeologist, McGregor Memorial Museum, Kimberley, Union of South Africa.

Correspondence

► In the examination of paleontological and anthropological skeletal material, its racial classification and biological reconstruction present important problems. Determining this for complete skeletons presents relatively few complications, but difficulties occur when only a single bone has been found. A solution of this problem is offered by the Coons fluorescence anti-

body technique (A. H. Coons and M. N. Kaplan, *Journal of Experimental Medicine*, 91:1-13, 1950), which was originally designed to permit the chemical labelling of an antibody with a fluorescent dye, without destroying its capacity to react specifically with its antigen in fresh tissue sections. In the Anatomical Institute at the University of Budapest we have developed a modification of this technique that is sensitive enough for decalcinated sections of fossil skeletal material. This makes it possible, even with a single bone, to determine the racial classification on the basis of the racial specificity of the organic material contained in the bone. With this method, more accurate classification and grouping may be possible than those based entirely on morphological examination. In optimal cases, blood group and Rh factor determinations as well as other paleoserological examinations could be made on human remains that would give extensive information about the relation or differentiation of various early hominid groups. From the paleontological point of view, suitable modifications of this method could establish the identification of prehistoric skeletal material with reference to already known classified findings.

Should any interest be shown in the above possibilities, I am ready to elaborate this modification of the Coons fluorescence antibody technique. I am publishing this note to establish cooperation with interested scientists in related fields.—DR. IMRE LENGYEL, 1st Department of Medicine, University Medical School, Korányi Sándor u. 2/a, Budapest VIII, Hungary.

► In connection with research on the Palaungs of Burma, I would appreciate any references (apart from those given in Embree and Dotson, *Bibliography of the peoples and cultures of mainland Southeast Asia*) to published or unpublished materials on any aspects of their culture.—CHARLES S. BRANT, Division of the Social Sciences, Portland State College, 1620 S.W. Park Avenue, Portland 1, Oregon.

Institutions

► Anthropological Institutions at the University of Veracruz, Mexico

Originally created in the form of the Office of Indian Affairs of the state of Veracruz, the INSTITUTE OF ANTHROPOLOGY of the University of Veracruz was successively converted into a Section of Anthropology and a Department of the Ministry of Education be-

fore achieving its present status. During all phases of its existence, this center of anthropological research has been concerned with the effects of government action on the biological, economic, cultural, and social conditions of communities, and with seeking better solutions for their problems. It has also contributed to the systematic knowledge of man and culture in Veracruz, as well as the preservation of historical data and cultural elements. The results are various types of publications as well as important collections of ethnographic, historical, osteological, and archeological materials.

As the development of anthropological field work rapidly enlarged the Institute's collections of cultural materials, a museum became indispensable. In 1957, the State Government and the University of Veracruz began construction of the MUSEUM OF ANTHROPOLOGY, which intends to present the development of man and culture in Veracruz from the most remote past to the present, covering a span of thirty centuries in the three principal culture areas of the State. Besides exhibition halls and a building for large monoliths, the Museum will eventually include a section within which will be built natural-size reproductions of various types of rural habitations, equipped with furniture and utensils characteristic of each ethnic group of the Commonwealth.

The SCHOOL OF ANTHROPOLOGY was established in 1947 as part of the Faculty of Philosophy and Letters of the University. Its purpose is the training of teachers of the anthropological sciences, and it specializes in archeology and social anthropology. Students are taught the techniques of anthropological research, with particular emphasis on understanding and resolving the problems of economically, socially, and culturally underdeveloped countries.

► Seminario de Integración Social Guatemalteca

In 1955, a group of social scientists who had specialized in Guatemalan studies, submitted to the government of Guatemala a project for a conference to discuss, compare, and synthesize their knowledge of Guatemalan society. They also hoped to formulate practical ideas for the social integration of a nation composed of elements that are socially and culturally diverse. Finally, they wished to use this opportunity to familiarize Guatemalans with the methods, theories, and results of the social sciences. The government approved and financed their proposal.

Eleven syntheses and seventeen commentaries on the papers were prepared for inter-disciplinary discussion at the

conference, held in June, 1956. The heart of the conference comprised a series of round-table discussions, each organized by specialists selected as discussion leaders. The discussion panels were entitled: Regional Integration; Integration in Social Structure; Integration of the Economic System; The Emerging National Culture; The Social Sciences in Public Administration; and Problems of Research and Teaching in the Social Sciences. Seventy persons in all (the majority of them specialists, Guatemalan or foreign) participated in the discussions.

During the closing session the Guatemalan government announced its intention to transform the temporary organization of the conference into a permanent Seminar. This offer has been partially fulfilled by the government, which appropriated interim funds allowing the Seminar to function for four years, but has not taken steps to legalize its position within the national administration. There is hope of the Seminar's achieving entry into public administration through an agreement with the president of the republic, as well as its obtaining an annual appropriation in the national budget, within the near future. The Seminar's editorial activities, its consulting services, and its efforts to further the concept of social integration as a means of facilitating the nation's progress and giving its inhabitants a more satisfactory system of human relations despite the persistence of ethnic differences, have created a favorable attitude toward it within Guatemala as well as among foreign scientists. This approval would facilitate its coalescence as a scientific institution and the extension of its activities in the future.

At present, the Seminar's principal activity consists of the publication, in Spanish, of studies of Guatemalan society written in foreign languages. By April 1960, it had published 17 scientific works of various types, principally social anthropology, with a total printing of approximately 3500 pages. Certain circumstances existing within the country have created other activities for the Seminar, including the organization of scientific and technical meetings in various fields. It also organized the Fourth Interamerican Indigenist Congress, held in Guatemala City in 1959. Other activities are of an advisory or consultative nature. Certain social welfare programs (especially those on education) have often requested the Seminar's help in the process of finding a resolution of their human problems. Several educational institutions and a large number of teachers have sought the Seminar's advice on matters relating to the teaching of social sciences. Al-

though the Seminar is not yet prepared to perform such services in an adequate fashion, the absence of Guatemalan organizations for instruction or research in the social sciences has made it impossible not to accept compromise which, qualified by many limitations, has produced deep and stimulating results.

If the Seminar is to survive and develop, its future plans must comprehend two essential activities related to the sciences of man. The first is the organization of a more or less formal service of applied anthropology to support the social welfare programs of the Guatemalan government. The second activity is an evaluation of the existing social science literature on Guatemala, with the object of determining the major lacunae and of filling these gaps through planned field research.

The conference materials were later collected in two books: *Integración Social en Guatemala* (1956), and *Integración Social en Guatemala, Volume II* (1960). The introduction to the former includes an appraisal by R. N. Adams of the material and focus of the work. In the latter appear general appraisals of the conference by J. Noval and J. L. Arriola which are recommended to persons interested in organizing similar meetings. Publications of the Seminar which may be obtained in exchange:

Cultura indígena de Guatemala. 2nd ed. 1959. (Short papers on various topics by R. N. Adams, J. Gillin, A. Goubault Carrera, O. La Farge, S. Mosk, B. D. Paul, R. Redfield, M. Siegel, S. Tax, and M. Tumin.)

FRANZ TERMER, 1957. *Etnología y etnografía de Guatemala*. (A survey of the material conditions, social life, knowledge and religion of the Guatemalan Indians, based on field materials collected over a period of almost 30 years.)

JOHN GILLIN, 1958. *San Luis Jilotepeque* (A community study of individual and social security in the culture of a town of Indians and Ladinos, with material on demography, land, economy, religion, social structure, political organization, and the relations between classes and "castes.")

OTTO STOLL, 1958. *Etnografía de Guatemala*. (A survey of 24 indigenous languages, with vocabularies, grammar and historical data on the peoples, in addition to a bibliographic ordering of the material and a map of linguistic distributions. The basic materials of the work were collected prior to 1883.)

Integración social en Guatemala, Volume II. 1960. (A résumé of the material of the 1956 volume, and transcriptions of discussions held by 6 round-tables of the 1956 conference.)

SOL TAX. *Capitalismo-de centavos*. (A community study which describes the processes of production, distribution and consumption of a small Indian town, as well as the forms and functions of wealth, economic behavior and decision-making processes, and the motivations of economic activity; it also contains much information about the quantitative economics of the small local society. The analysis is purely anthropological. [In press])

Further information may be obtained from the Editor, JORGE LUIS ARRIOLA, Seminario de Integración Social Guatemalteca, Guatemala, A.C.

► The AMERICAN INDIAN ETHNOHISTORIC CONFERENCE is an association of persons and organizations interested in historical studies of cultures and peoples that are normally considered to be outside the tradition of Western European culture. Its membership, international in distribution, includes historians, archaeologists, and anthropologists, as well as universities and libraries.

The Conference sponsors *Ethnohistory*, a quarterly journal devoted to original historical research on the cultures and movements of non-Western peoples, primarily as these are traced through documentary materials. Although the journal is not specialized regionally, it emphasizes New World studies. Published by Indiana University in February, May, August, and November, *Ethnohistory* includes original articles, edited manuscripts, and reviews. Each annual volume contains around 400 pages. Publication was begun in 1954. It is also planned to issue occasional contributions, which because of length or for other reasons do not conform to the journal format. The Conference also publishes a *Newsletter* twice a year, announcements of interest and news of members.

The Conference also holds an annual meeting, at which papers are presented and discussed.

Serial Publications

► *Kölner Ethnologische Mitteilungen*, a new series of ethnographical publications, has been established by the SEMINAR FÜR VÖLKERKUNDE at the University of Cologne, under the direction of H. Petri. Volume 1, "Beiträge zur Ethnographie des Wantat Tales in Nordost Neuguinea," by Carl A. Schmitz, appeared in December, 1960. Volume 2, "Beiträge zur Ethnographie der Pala, Mittel Neuirland," by P. Karl Neuhäus, edited by P. C. Laufer and Carl

A. Schmitz, will be issued in 1961.

► *Art in Its Context* is a new series of studies in ethno-aesthetics, edited by two Dutch scholars, A. A. Gerbrands and F. Sierksma, in co-operation with colleagues from various countries. In each volume, the art of one culture or culture-area will be treated. Emphasis will be given to the socio-cultural context of art, while the role and function of the individual artist, as well as problems of style, will receive the attention they deserve. In general, the art of ethnological peoples will be dealt with, although the art of urban cultures will not be neglected. The volumes that concern the art of still-living peoples will be written by authors who have carried out field-work, and preferably by anthropologists who have a special interest in ethno-aesthetics. At the moment, one of the editors is in southwest New Guinea, studying the art of the Asmat area.

The editors and the authors would like to provide scholars, who are interested in the relation between art and other aspects of culture, with detailed data concerning specific cultures. In this way, a more systematic and comprehensive approach to ethno-aesthetics might be achieved. Although art is not directly necessary for cultural survival, it may prove to have an important part in the stabilization and continuation of a culture, whatever its overt functions may be. However, very little is known in this field; and consequently every contributor has complete liberty to express his views, after he has given the facts. The average volume will number 180 pages, with about 60 full-page photographs, some in color.

The editors discovered that several field-workers are in possession of valuable material, but lacked opportunity for its adequate printing and reproduction. Similarly circumstanced scholars, who might be interested in publishing material, are invited to write to Dr. F. SIERKSMA, Mariënpoolstraat 2, Leiden, The Netherlands. Comments and suggestions will be welcome. The editors would be particularly pleased to contact a colleague who might write a study of the various effects of acculturation on the art of a community.

Recommended to Read

I should like to call the attention of readers of *CA* to the following publication:

Second conference on history and archaeology in Africa, London, 1957. Edited by D. H. Jones. School of Oriental and African Studies, London, 1959.

58 pp.

The work of the conference explicitly breaks away from the conception of African historiography as condescending "Old Coaster's memoirs" or descriptions of the oriental splendors of early royal courts. It aims instead at the rapidly growing demand from the literate African public for historical accounts of their own past—treated not as episodes in European colonial enterprise, but as interaction of European, Islamic, and other influences with strong and continuing indigenous cultural traditions. In doing so, its repertoire of techniques includes, besides archaeology and archival history, such other aspects of a broadly anthropological approach as serology, glottochronology, dendrochronology, and the collection and critical study of oral traditions.

While I am not qualified to review its substantive contributions, it seems to me that this slender report of plenary discussions embodies one of the most salutary trends in our field. Modestly, and with full respect for the limitations of present data and the immense work that lies ahead, it sets its hand not to the endless refinement of self-derived problems but instead to the newly emergent needs of African societies with whose future growth our own future is more and more closely linked. In the process, it supports and illustrates the essential unity of our discipline more effectively than programmatic pronouncements ever can.

ROBERT M. ADAMS
Chicago, Ill., U.S.A.

The attention of anthropologists is called to the publication of *Man, Race and Darwin*, consisting of the papers read at a conference held at the Royal Anthropological Institute in 1959, jointly with the Institute of Race Relations. All aspects of race are covered and brought up to date by experts in the United Kingdom. Copies may be obtained from the Oxford University Press, Amen House, Warwick Square, London, E.C.4.

ROYAL ANTHROPOLOGICAL INSTITUTE
London, England

For Sale

► A third edition of George P. Murdock's *Ethnographic Bibliography of North America*, which has been out of print for some time, has been issued by the Human Relations Area Files, P.O. Box 2054, Yale Station, New Haven, Conn., U.S.A. Prepared in response to many requests, the new edition has been considerably enlarged (pp. xxiv, 395; 15 maps). Its price is U.S.A. \$6.75.

HRAF also offers a set of file slips,

Summaries of African Tribes and Peoples, by George P. Murdock. The slips cover all of Africa and present essential social, cultural, and economic information, as well as a selected bibliography for each of approximately 1000 different ethnolinguistic groups. Issued primarily for HRAF's member institutions, one hundred additional sets were added to accommodate anthropologists and African study programs. Since only a hundred sets are available, and more cannot be printed, those interested are urged to order promptly. The price is U.S.A. \$20.00 per set, boxed.

► *Lake habitations and pre-historic remains in Italy*, by Bartolomeo Gastaldi, published by the Anthropological Society of London in 1865. The Royal Anthropological Institute has six copies (unbound), offered for sale at 10 shillings each. Persons or libraries interested in purchasing these should write the Publications Secretary, 21 Bedford Square, London, W.C. 1, England.

Congresses

► Working Section 10 of the VIth International Congress of Anthropological and Ethnological Sciences, held in Paris, August 1960, was devoted to a discussion of food, drink, and bread, particularly ritual breads and their uses; and to the establishment of a clearing house where information and questions about these topics may be directed. For working purposes, the group decided to separate its subject into three groups: (1) Food and drink in general; (2) Ritual and common breads of Europe and the Near East; and (3) Ritual and common breads of all other parts of the world.

All information and questions concerning the breads of Europe and the Near East should be directed to Dr. Max Währen, Alemannenstr. 21, Bern 18, Switzerland. Questions and information concerning the other two divisions should be sent to the chairman of the session, MME. GERMAINE DIETERLEN, % Musée de L'Homme, Paris 16, who will forward them to the appropriate departments. (Information supplied by MARGARET L. ARNOTT.)

Conferences

► **Economics and Anthropology (Capital, Saving, and Credit in Peasant Societies)**

August 22-28, 1960, at Burg Wartenstein, Austria.

Sponsored by The Wenner-Gren Foundation for Anthropological Research, Inc.

Organizing Chairmen: Raymond Firth, London School of Economics and Political Science, University of London, England; and Bert F. Hoselitz, Research Center in Economic Development and Cultural Change, University of Chicago, Chicago, Ill., U.S.A.

Participants, and their papers:

F. G. BAILEY, School of Oriental and African Studies, University of London. "Capital, saving, and credit in highland Orissa."

FREDRICK BARTH, Box 37, Voksenkollen, Norway. "Capital and investment, and the social structure of a pastoral nomad group."

CYRIL S. BELSHAW, University of British Columbia, Vancouver, B.C., Canada. "Institutions for capital formation and distribution among Fijians."

ERIC DE DAMPIERRE, Centre d'Études Sociologiques, C.N.R.S., Paris, France. "Coton noir, café blanc. Deux cultures du Haut Oubangui à la veille de la loi-cadre."

RAYMOND FIRTH, London School of Economics and Political Science, University of London. "Problems of economic anthropology: Formation and maintenance of capital in peasant societies."

BERT F. HOSELITZ, Research Center in Economic Development and Cultural Change, University of Chicago. "Capital formation, saving, and credit in Indian agricultural society."

LORRAINE LANCASTER, London School of Economics and Political Science, University of London. "Some aspects of credit, saving, and investment in a 'non-monetary' economy."

SIDNEY MINTZ, Department of Anthropology, Yale University, New Haven, Conn., U.S.A. "The employment of capital by market women in Haiti."

MICHAEL SWIFT, Department of Malay Studies, University of Malaya in Kuala Lumpur, Federation of Malaya. "Capital, saving, and credit in Malay peasant economy."

BASIL YAMEY, London School of Economics and Political Science, University of London. "Capital and credit in peasant societies: Some economic questions."

Other participants:

RUDOLF BIČANIĆ, Department of Sociology, University of Zagreb, Yugoslavia.

ANDREW GUNDER FRANK, Monteith College, Wayne State University, Detroit, Mich., U.S.A.

HENRI MENDRAS, Centre d'Études Sociologiques, C.N.R.S., Paris, France.

CHIE NAKANE, Institute for Oriental Culture, University of Tokyo, Japan.

ZOFIA SZYFELBEJN, Department of Ethnology, University of Warsaw, Poland.

SOL TAX, Department of Anthropology, University of Chicago, Chicago, Ill., U.S.A.

JOAN THIRSK, Department of English Local History, University of Leicester, England.

Other papers discussed:

BURTON BENEDICT, "Capital, saving, and credit among Mauritian Indians."

ALICE DEWEY, "Capital, credit, and savings in Javanese marketing."

SCARLETT EPSTEIN, "Personal capital formation among the Tolais of New Britain."

JOEL M. HALPERN, "Capital, savings and credit among Lao and Serb peasants—a contrast in cultural values."

JOAN METGE, "Rural local savings associations (Maori komiti) in action in New Zealand's far north."

MANNING NASH, "Capital, saving and credit in a Guatemalan and Mexican Indian peasant society."

RAYMOND SMITH, "Ethnic differences in economic activity in British Guiana."

Report from Chicago Conference, *Workshop in Economic Anthropology*, September, 1959.

Discussion:

Only a brief account of discussion is given here; it is expected that a full examination of arguments and conclusions will be published—probably by the journal *Economic Development and Cultural Change* in 1961.

To some extent this conference built—as it was intended to—on the discussion and results of an earlier conference held in Chicago, which had dealt with problems of co-operation between economists and anthropologists in the field of economic development (see report, 'Workshop in Economic Anthropology,' CURRENT ANTHROPOLOGY, March, 1960, p. 149). Though the terms of reference of the second conference were narrower than those of the first—capital, saving and credit in peasant societies being its sub-heading—discussion turned frequently to fundamental questions of the feasibility and desirability of joint anthropological-economic research.

Nevertheless, some specific fields of enquiry were explored, using material derived from the data papers and from the research of individual participants. These fields were defined as: (1) concepts of capital, saving and investment and their application to peasant societies; (2) capital accumulation and social structure; (3) management of capital by agriculturalists, pastoralists, and traders; (4) general problems of credit in

peasant societies; and (5) types of saving and investment.

Some topics emerged repeatedly in different contexts, particularly what was implied by "peasant economy." It was agreed that "peasant" could not be equated only with rural or agricultural workers, and several dimensions of a possible definition were examined. Type of settlement, residence, and relationship to the land; type of links of the "peasant" economy with town, city, and outside world, and the effect of complete or partial monetization were important, as was the fact that in a peasant society there are recognized roles that can be conceived of in economic terms only.

Discussion of the problem of identification of capital in peasant societies led to categorization of major items of peasant capital in the form of houses, land (there is an argument in this context for setting aside the classic distinction between land and capital), agricultural and other implements or equipment, domestic animals, jewelry, stocks (e.g. of grain and trade goods), cash (loans, etc.), rights which command services or yield income and social capital in the forms of roads, bridges, etc.

The effect of small-scale production was relevant to many topics such as, for instance, the assessment or the cost of capital maintenance in societies with relatively low productivity. Some other topics discussed concerned the relationship of external political factors to the peasant economy, and their impact on opportunities open to entrepreneurs; the circumstances under which it might be necessary for an entrepreneur to break with his existing social network; the pressures operating against innovation in the use of capital; the effect of the burden of various rates of interest on loans in peasant societies; the advantages of sectoral analysis in examining the relationship between economy and other aspects of the social structure; and the way in which Western institutions (e.g., banks) may change when transplanted to underdeveloped countries.

Results:

Discussion appeared to result in clarification, and in suggestion of some new ways of looking at material; on the whole it seemed to reinforce the conviction held by most participants that there was a very wide field, in the study of development, where economics and anthropology could profitably be brought together, by one worker with expertise in both disciplines, or by a team combining economists and anthropologists, or by the taking of opinion on specific topics where anthropologists and/or economists could be called in as outside advisers.

In terms of practical beginnings, participants attempted to formulate, as hypotheses or themes, notions that were implicit or explicit in discussion and papers presented. A few of the more self-explanatory of these are given here in the hope that they may be tested as they stand, or put forward in more testable form, by those interested in the field.

It seems possible that:

(1) Where there is great differentiation in farm size and wealth, and where there is no governmental credit available on favourable terms to the poorest peasants and where there are no outside sources of income, credit given by the rich peasants to the poor for the purpose of consumption:

- a) is the source of social control;
- b) preserves the small farms and also preserves labour forces;
- c) is a form of capital accumulation but does not affect existing social differentiation.

(2) Where there is great differentiation in farm size and wealth, and where there is no governmental credit available on favourable terms to the poorest peasants, the rich peasants do not lend money to the poor (at least not from the same village). In consequence of which:

- a) the rich peasants invest money outside;
- b) the former cultural unity of the community disappears, the rich clinging to the traditional ways while the poor adopt the culture of the town;
- c) the rich lose social control over the poor, and the values on which traditional leadership was based change;
- d) there is no change, however, in farm size and wealth.

(3) In a society where kinship ties beyond the immediate family do not necessarily involve obligation, the greater the gulf of wealth between lender and borrower and the higher the degree of integration of the rich peasant group, then the stricter is the execution of debts.

(4) In these circumstances, rich farmers are concerned with lending money to medium-size farmers. Medium-size farmers borrow money from them until:

- a) government credit is made available for them on favourable terms;
- b) social control by the rich peasants becomes less powerful;
- c) the group of poor peasants has the opportunity of getting income from outside sources;
- d) the rich group of peasants loses the labour resources of the poor both within and without the community.

Possible themes for further investigation concern:

(1) The relationship between the rate of capital accumulation and the type of social differentiation in a peasant society;

(2) The periodicity of income (i.e., regular or irregular and seasonal) and its relation to the case of capital accumulation;

(3) The possible effects of attempts to use traditional forms of co-operation to introduce new methods in agriculture (c.f., policy in some Communist countries to attempt to destroy old forms and introduce new ones);

(4) The ways in which rituals and ceremonies may be related to saving and consumption;

(5) The differential effects of types of taxation on social structure and capital accumulation;

(6) The possible limiting effects on the building-up of capital of segregated spheres of exchange in an economy;

(7) The effects on the network of credit extension of different descent and residence systems, in the case of a large-scale market invasion of a territory.

The papers presented at the conference will be collected and published as a volume edited by Raymond Firth and Basil Yamey, entitled *Capital, Saving and Credit in Peasant Societies*.

Communications concerning the conference may be sent to Lorraine Lancaster, London School of Economics.

Rapporteur: Lorraine Lancaster

► Visual Communications

November 3-5, 1960 at New York City, N.Y., U.S.A.

Sponsored by The Wenner-Gren Foundation for Anthropological Research, Inc.

Chairmen: Dr. Stephan F. Borhegyi, Director, Milwaukee Public Museum, Milwaukee, Wis., U.S.A.; and Lothar P. Witteborg, Exhibit Consultant, Brooklyn, N.Y., U.S.A.

Participants:

STEPHAN F. BORHEGYI

DUNCAN F. CAMERON, Office of Information Service, Royal Ontario Museum, Toronto, Canada.

FREDERICK DOCKSTADER, Director, Museum of the American Indian, Heye Foundation, New York, N.Y., U.S.A.

LOUIS DORFSMAN, Creative Director, Advertising and Promotion, C.B.S. Television, New York, N.Y., U.S.A.

HENRY GARDINER, Senior Designer, American Museum of Natural History, New York, N.Y., U.S.A.

EDWARD GREEN, Artist, Milwaukee Public Museum, Milwaukee, Wis., U.S.A.

H. PHILIPP HEMMINGSEN, Director of Design, Gorham Silver Company, Providence, R.I., U.S.A.

ROBERT BRUCE INVERARITY, Director, Adirondack Museum, Blue Mountain Lake, N.Y., U.S.A.

CARL MAAS, Art Director, Standard Oil Company of New Jersey, New York, N.Y., U.S.A.

WALLACE N. MACBRIAR, JR., Chief, Office of Information Services, Milwaukee Public Museum, Milwaukee, Wis., U.S.A.

TAIZO MIAKE, Instructor of Design, Layton School of Art, Milwaukee, Wis., U.S.A.

ALBERT PARR, Senior Scientist, American Museum of Natural History, New York, N.Y., U.S.A.

LEE A. PARSONS, Assistant Curator of Anthropology, Milwaukee Public Museum, Milwaukee, Wis., U.S.A.

FROELICH RAINEY, Director, University Museum, University of Pennsylvania, Philadelphia, Penna., U.S.A.

ROBERT RITZENTHALER, Curator of Anthropology, Milwaukee Public Museum, Milwaukee, Wis., U.S.A.

MARTIN ROSENZWEIG, Art Director, Coronet Magazine, New York, N.Y., U.S.A.

LOUIS SILVERSTEIN, Art Director, New York Times Promotion, New York, N.Y., U.S.A.

W. STEPHEN THOMAS, Director, Rochester Museum of Arts and Sciences, Rochester, N.Y., U.S.A.

ALEX WILLIAMS, Chief, Exhibition Department, American Museum of Natural History, New York, N.Y., U.S.A.

LOTHAR P. WITTEBORG

Discussion:

In this conference, museum directors and experts in commercial presentation and communication gathered to exchange ideas and explore new avenues of museum presentation. Several broad areas were delineated for discussion by the group.

The relationship of museum design to commercial design was the first of these areas to be taken up. After establishing a groundwork of agreement on the purpose and function of museums, their influence on the industrial world and the various contributions museums can make to this wider public were discussed. Other points that interested the conferees were: the contributions of museums to the ever-increasing leisure time of Americans, whether museums should be used as centers of entertainment and public education or as centers of scholarly research, and the points of view of museum visitors.

Visual communication was another broad area explored by conference members. Discussion of the techniques and design factors that contribute to a good exhibition program centered around subject matter, form and space, color, light, sound, and the written

word. Ideas versus objects (or products), the designing of meaningful and storytelling exhibits, and the usefulness of testing devices and visitor surveys in evaluating the effectiveness of display techniques, were other aspects of visual communication covered. Mutual assistance between companies and museums, and the value of public relations techniques were also considered.

Results:

The participants agreed that more meetings of this type would be useful, and that the points of view of persons who work on display and presentation problems outside the museum were most beneficial to the latter. The entire discussion was recorded on audio tapes, and will later be published.

► Symposium on Comparative Aspects of Human Communication

September 4-10, 1960, at Burg Warstein, Austria.

Sponsored by the Wenner-Gren Foundation for Anthropological Research, Inc.

Organizing Chairman: Godfrey E. Arnold, National Hospital for Speech Disorders, New York, U.S.A.; in collaboration with John Lotz, Columbia University, New York; and George Kelemen, Harvard Medical School, Massachusetts Eye and Ear Infirmary, Boston, Mass., U.S.A.

Participants:

GODFREY E. ARNOLD.
 MACDONALD CRITCHLEY, National Hospital, London, England.
 L. CROATTO, Centro Medico di Foniatria e di Rieducazione Acustica, Padova, Italy.
 GUNNAR FANT, KUNGL. Tekniska Högskolan, Institutionen för Telegrafitelefoni, Stockholm, Sweden.
 F. GREVEL, Universiteit van Amsterdam, Netherlands.
 DONALD R. GRIFFIN, Harvard University, Cambridge, Mass., U.S.A.
 LYNWOOD HEAVER, National Hospital for Speech Disorders, New York, U.S.A.
 CHARLES F. HOCKETT, Cornell University, Ithaca, N.Y., U.S.A.
 GEORGE KELEMEN.
 ALFRED L. KROEBER, University of California, Berkeley, Calif., U.S.A.
 MARTIN LINDAUER, Zoologisches Institut der Universität München, Germany.
 JOHN LOTZ.
 THOMAS A. SEBEOK, Indiana University, Bloomington, Ind., U.S.A.
 ANTONIO SUBIRANA, Universidad de Barcelona, Spain.
 JW. VAN DEN BERG, Physiologisch Laboratorium der Rijks Universiteit, Groningen, Netherlands.

FRITZ WINCKEL, Technische Universität Berlin, Berlin-Charlottenburg, Germany.

EBERHARD ZWIRNER, Institut für Phonetik Universität Münster, Germany.

Discussion:

Providing opportunities for discussion among specialists who share a common interest in the study of communication, the symposium brought together participants from the fields of linguistics, anthropology, phonetics, acoustics, zoology, neurology, psychiatry, and otolaryngologic physio-pathology. Their diversity of orientation, terminology, and interests ensured a wide range of detailed information, although it did not always facilitate communication.

All participants were especially interested in the communication systems of animals, including the "language" of bees, which was demonstrated to be more similar to human language than had previously been realized. Several participants advocated systematic studies of the comparative aspects of animal communication systems, and the adoption of concepts which recognize the structural framework and function of the peripheral and central neuromuscular phonation apparatus of the species. It was agreed that cautious avoidance of anthropomorphic viewpoints and a large amount of detailed study is needed to achieve a better understanding of communication among animals, which may then be utilized for clarification of human communication.

Major interdisciplinary definitions of the basic concepts in human communication research were considered premature; and the main achievement of the symposium was a tendency toward the synthesis of divergent viewpoints, which may later lead to a fruitful analysis of details and specific problems. Participants representing technical studies agreed that the fusion of multidisciplinary viewpoints inspired by anthropological methods will lead to broader concepts of communication theory.

At the last session, arrangements were made to publish the proceedings of the symposium in *Logos*, which will be made available to the anthropological audience. Since it is clearly impractical to unite such divergent groups for the solution of specific problems, the informal organization of a permanent committee on comparative aspects of communication was moved. It was also voted that a specific meeting for the concrete and detailed planning of further studies should be convened by investigators who are interested in the systematic exploration of animal "language." Every partial solution of per-

inent problems is a step toward the goal of a better understanding of the essence of human communication and its phylogenetic evolution.

List of Films Shown:

D. R. Griffin: 1) Tape recordings of the orientation sounds of various kinds of bats "translated" into the range of human hearing by playback at reduced speed. Copies obtainable by correspondence. 2) 16 mm. sound film showing bats intercepting flying insects: Part I, at natural speed with orientation sounds monitored crudely by rectification; and Part II, slow motion pictures, with synchronized reduced speed sound track, of bat catching meal worms tossed into air. Prepared by Mr. F. Webster, 62 Coolidge Ave., Cambridge 38, Mass., U.S.A. 3) 16 mm. sound motion picture of porpoises engaged in echolocation, by Dr. Kenneth Norris, Dept. of Zoology, University of California, Los Angeles, Calif., U.S.A., and Mr. Michael Sklar, Columbia Broadcasting Company, New York, N.Y., U.S.A.

Dr. Jw. van den Berg, Head, Department of Medical Physics, Physiological Laboratory, University of Groningen, Netherlands: 1) "Laryngeal vibrational patterns," film on human and animal larynxes, black and white, 15 min. 2) "Voice production, the vibrating larynx," film on structure and function of the human larynx, black and white, 42 min., sound. Distributed by SFW Catharine Singel, 59, Utrecht, Netherlands. 3) "Oesophageal speech," film on the mechanism of oesophageal speech after total laryngectomy with the aid of X-ray image intensifier, black and white, sound, 12 min. Distributed by Philips N.V., Eindhoven, Netherlands. "Speech work at the Haskins Laboratories." (Inquiries: Dr. Franklin S. Cooper, President, Haskins Laboratories, 305 East 43rd St., New York 19, N.Y., U.S.A.)

"Some aspects of the speech event," by Drs. Ramsay, Gramiak, Watson, Weinberg (University of Rochester), Dr. Cooper (Haskins Laboratories), and Dr. Lotz (Columbia University). (Inquiries: Dr. John Lotz, 701 Philosophy Hall, Columbia University, New York 27, N.Y., U.S.A.)

"Talking hands" (on the Indian Sign Language) by Laubin (Inquiries: The Wenner-Gren Foundation, 14 East 71st St., New York 21, N.Y., U.S.A.)

► From 15,000 B.C. to the Thresholds of Urban Civilization—A World-Wide Consideration of the Cultural Alternatives

July 3–11, 1960, at Burg Wartenstein, Austria.

Sponsored by The Wenner-Gren Foundation for Anthropological Research, Inc.

Organizing Chairmen: Robert J. Braidwood, Oriental Institute and Dept. of Anthropology, University of Chicago, Chicago, Ill., U.S.A.; and Gordon R. Willey, Peabody Museum and Dept. of Anthropology, Harvard University, Cambridge, Mass., U.S.A.

Participants, and their papers prepared and exchanged in advance:

ROBERT J. BRAIDWOOD and BRUCE HOWE, Peabody Museum. "Southwestern Asia—east of the Euphrates Drainage—from about 15,000 B.C. to about 4,000 B.C."

JOSEPH R. CALDWELL, Illinois State Museum, Springfield, Ill., U.S.A. "Eastern North American Prehistory."

KWANG-CHIH CHANG, Peabody Museum. "China, from 15,000 B.C. to the thresholds of civilization."

J. DESMOND CLARK, The Rhodes-Livingstone Museum, Livingstone, Northern Rhodesia. "From food collecting to incipient urbanization in Africa south of the Sahara."

DONALD COLLIER, Chicago Natural History Museum, Chicago, Ill., U.S.A. "To the threshold of civilization in the central Andes."

EMIL W. HAURY, University of Arizona, Tucson, Ariz., U.S.A. "The road to urbanism in the greater American Southwest."

BOHUSLAV KLÍMA, Československá Akademie Věd Archeologický Ústav, Brno, Czechoslovakia. "Der Erste Grundriss Einer Jungpaläolithischen Lössiedlung in Mitteleuropa und Ihre Bedeutung."

CARL-AXEL MOBERG, Arkæologiska Museet, Göteborg, Sweden. "On food collecting—food production—incipient urbanization in Baltic Europe."

JEAN PERROT, Mission Archéologique Française en Israël, Jerusalem, Israel. "From food collecting to incipient urbanization in Palestine, Syria and Cilicia."

RICHARD PITTIONI, Urgeschichtliches Institut, Wien, Austria. "Von 15.000 V.Ch.G. Bis Zur Schwelle der Stadtzivilisation Südliches Mitteleuropa und Südosteuropa."

IRVING ROUSE, Yale University, New Haven, Conn., U.S.A. "Development of culture in the Northern Andes, Amazonia, and the Caribbean Area."

HERMANN SCHWABEDISSEN, Institut für Ur- u. Frühgeschichte der Universität, Köln, Germany. "Vom Jungpaläolithikum Bis Zum Beginn Des Neolithikums Im Nördlichen Kontinentaleuropa."

H. T. JALLING WATERBOLK, Rijksuniversiteit, Biologisch-Archaeologisch Instituut, Groningen, Holland. "The lower Rhine Basin."

GORDON R. WILLEY. "Courses to the threshold of civilization: Mesoamerica."

Those who prepared papers but were unable to attend the symposium:

A. P. OKLADNIKOV, Institute of Archeology, Leningrad, U.S.S.R. "Archeology east of the Urals."

HASMUKH D. SANKALIA, Deccan College Postgraduate and Research Institute, University of Poona, India. "From food collection to urbanization in India."

Discussion:

The purpose of the symposium was set forth in a preliminary letter of invitation sent to the participants in the fall of 1959.

The symposium was to concern itself with the tracing of man's history from latest Pleistocene times up to the threshold of the urban civilizations. This was projected upon a world-wide basis, and was to deal, substantively, with those archeological evidences which reveal the varying degrees of intensification of food-collecting; the transitions from food-collecting to partial or to fully effective food-production; and the eventual emergence of city life and civilization. The cultural consequences and accompaniments of these transitions were to be examined closely. Inquiry was to be directed not only to those regions where urbanization first crystallized, but to more "peripheral" regions which may or may not have attained full urbanism.

Attention was to be given to environmental adaptations under differing conditions, and to shifts in adaptations either before or following the appearance of food-production. The relative roles of environmental factors and migration and/or diffusion in the conversion of hunter-collector cultures to those of food production were also to be studied. Interest also centered upon the qualities and quantities of cultural intensifications immediately antecedent to the appearance of urban civilizations, analyzing the evidence for settlement patterns, population sizes and groupings, long range trade, incipient "priest-hoods," "kingships," and the institution of warfare. In brief, the question was posed: What can the prehistoric archeologist contribute to the understanding of why urban civilizations came about when and where they did?

As a working hypothesis, the delineations of an urban civilization given by Childe (*Town Planning Review* 21: 3–17, 1950), with Redfield's comments in *The Primitive World and Its Trans-*

formations (1953), were used. The goal of the symposium was not so much the definition of urban civilization as such—or a *post facto* analysis of its genesis via “historic” materials—but rather a consideration of the varieties of cultural build-ups leading to the thresholds of urban civilizations.

The organization of the symposium began with the preparation of the above series of background papers by the participants. These were statements bearing upon the archeology of particular world areas with reference to the general theme of the symposium. All were relatively brief; none exceeded 10,000 words. These papers were then duplicated and circulated among all participants in advance of their arrival at Burg Wartenstein.

The symposium opened with 3½ days of panel discussions based upon the background papers. Discussion ranged over possibilities of generalizations about culture change in widely separated world areas. “Ecologically” similar culture areas were examined, as well as areas which provided strong environmental contrasts. Similarly, “nuclear” and “marginal” area developments were explored for parallels and differences. A day of unprogrammed discussion followed, during which time five central questions were formulated:

(1) In the late glacial and early post-glacial periods, what major cultural events characterize your area? By what archeological traces are these expressed?

(2) Defining incipient cultivation (and/or animal domestication) as a minor or supplementary basis of total subsistence—when and how do such conditions appear?

(3) At what point in the cultural sequence of your area do you feel that you can identify effective food production (plant cultivation and/or animal domestication assuming a major subsistence role) and what are its artifactual expressions and social (directly inferred) consequences?

(4) Does effective food-production appear as part of an indigenous evolution, or does it (as revealed archeologically) suggest outside influences? To what extent does the appearance of effective food-production (either indigenous or imported) seem explosive (“revolutionary”)?

(5) Could you use the term “threshold of urbanization” in your area? If so, what would you mean, and what is the evidence for its development?

The remaining four working days of the symposium were devoted to the reworking and redrafting of the background papers in the light of these “thematic” questions. This was done; but in its course many informal talks on all aspects of the symposium subject

were held among members of the group.

The final versions of the background papers, with an introduction and some concluding remarks by the editors, R. J. Braidwood and G. R. Willey, will be published in the near future, probably as a Viking Fund Publication in Anthropology.

► Early Man and Pleistocene Stratigraphy in the Circum-Mediterranean Regions

July 14–20, 1960, at Burg Wartenstein, Austria.

Sponsored by The Wenner-Gren Foundation for Anthropological Research, Inc.

Organizing Chairmen: Alberto Carlo Blanc, Università di Roma (Città Universitaria), Rome, Italy (deceased July 4, 1960); and F. Clark Howell, University of Chicago, Chicago, Illinois, U.S.A.

Participants, and their papers prepared and exchanged in advance:

BRUNO ACCORDI, Istituto di Geologia e Paleontologia, Università di Roma. “Some data on the Pleistocene stratigraphy and pigmy mammalian faunas of eastern Sicily.”

C. ARAMBOURG, Muséum National d'Histoire Naturelle, Paris. “Les faunes mammalogiques du Pléistocène circumméditerranéen.”

M. AVNIMELECH, Department of Geology, Hebrew University, Jerusalem. “The latest history of the Mediterranean coast of Israel.”

LIONEL BALOUT, Université d'Alger et Musée du Bardo, Algiers. “Le Paléolithique de l'Algérie.”

PIERRE BIBERSON, Musée de l'Homme, Paris. “L'évolution du Paléolithique marocain dans le cadre du Pléistocène atlantique.”

A. C. BLANC, (deceased prior to conference). “Sur le Pléistocène marin des côtes Tyrrhéniennes et Ioniennes et les cultures Paléolithiques associées.”

EUGENE BONIFAY, Institut de Paléontologie Humaine, Paris. “Quaternaire et préhistoire des régions méditerranéennes françaises.”

KARL W. BUTZER, Department of Geography, University of Wisconsin, Madison, Wis., U.S.A. “Pleistocene stratigraphy and prehistory in Egypt.”

GILBERT CASTANY, Bureau des Recherches géologiques et Minières, Paris. (Paper to be completed after date of conference: Pleistocene stratigraphy of Tunisian littoral.)

G. CHOUBERT, Service Géologique du Maroc, Rabat, Morocco. “Reflexions sur les parallélismes probables des formations Quaternaires atlantiques du Maroc avec celles de la Méditer-

ranée.”

RHODES W. FAIRBRIDGE, Department of Geology, Columbia University, New York, U.S.A. “World sea-level and climatic changes.”

H. FLEISCH, Université Saint Joseph, Beyrouth, Lebanon. (Paper to be prepared: on older Pleistocene marine levels of coastal Lebanon.)

D. A. E. GARROD, Villebois-sur-Lavalette, France. “Prehistoric data: Palestine-Lebanon.”

M. GIGOUT, Laboratoire de Géologie, Université de Lyon, France. “Sur le Tyrrhénien de la Méditerranée occidentale.”

E. G. GOBERT, Muséum d'Histoire Naturelle d'Aix-en-Provence, France. “Prehistory in the Tunisian coastal area.”

R. W. HEY, Department of Geology, University of Cambridge, England. “The Quaternary geology and Paleolithic archaeology of Libya (excluding the Fezzan).”

F. CLARK HOWELL, (Organizing Chairman).

RAIMONDO SELLI, Istituto di Geologia e Paleontologia, Università di Bologna, Bologna, Italy. “Le Quaternaire marin du versant Adriatique-Ionien de la péninsule italienne.”

LUIS SOLÉ SABARÍS, Instituto Geológico, Universidad de Barcelona, Barcelona, Spain. “Le Quaternaire marin des Baléares et ses rapports avec les côtes méditerranéennes de la péninsule Ibérique.”

H. E. WRIGHT, JR., Department of Geology and Mineralogy, University of Minnesota, Minneapolis, Minn., U.S.A. “Late Pleistocene geology of coastal Lebanon.”

Discussion:

The greater part of the discussion concerned the nature and interpretation of the marine and varied intercalated continental sediments comprising the Pleistocene succession along the continental margins adjacent to the basin of the Mediterranean Sea and the Atlantic littoral of Morocco.

Notable evidence of the marine later Pliocene is provided by the Tyrrhenian, and especially the Adriatic, coasts of Italy. The Lower Pleistocene is represented by superposed marine sediments of variable facies, sometimes continuous, and sometimes disconformable; these are the transgressive Calabrian (including the regressive Emilian) and the Sicilian stages. Where the marine Calabrian is continuous (non-transgressive) with the Pliocene, the transition between Pliocene and Lower Pleistocene may be indicated by climatic oscillations. A major stage, surely complex and subdivisible on both stratigraphic and faunal grounds, which

is termed the Milazzian (after the definition by Depéret), and characterized by an indifferent invertebrate fauna, is of Middle Pleistocene age and is generally transgressive on these sediments. In the Northern Apennines near the Po Valley, intercalated continental sediments with an early Abbeville-Acheulian industry are known. These earlier stages are overlain, at places with erosional unconformity, by marine (Tyrrhenian complex) and continental sediments which evidently represent a considerably shorter range of time. Such factors as subsidence (Po delta), orogeny, and vulcanism, which played distinctive roles in particular regions, often complicate the interpretation and obscure the sequence of marine and continental sedimentation. In the Rome area, however, differences in times of volcanic activity and in eruptive products among particular groups of volcanoes have served as helpful markers in distinguishing various stages of marine and continental sedimentation; some of these have been shown to be dateable by isotopic (potassium/argon) methods. Paleontological and palynological studies afford some evidence of climatic changes during and subsequent to the continental Villafranchian.

The Atlantic littoral of Morocco, an area which, though uplifted during the Pliocene and Pleistocene, remained undeformed during the Pleistocene clearly provides one of the most complete Pleistocene successions in the world. At least five (and perhaps six) distinct marine cycles, designated according to a local terminology, have been distinguished, and are related to continental formations—often with mammalian fossils, and implement-bearing—which extend from the earliest Pleistocene (continental Villafranchian) through the final Pleistocene. An extraordinary succession of lithic industries is closely dated with reference to this sequence. It begins with four stages of the so-called "Pebble-Culture" (Pre-Chelles Acheul in sub-Saharan Africa), the last of which is transitional to the oldest bifacial industry. These are followed by eight distinct, and both technically and typologically changing, Acheulian industrial stages; succeeded in turn by a (perhaps) Mousterian of Acheulian facies, the Aterian industry of the later Upper Pleistocene, and the end- and post-Pleistocene-Ibero-Maurusian (Oranian) industry.

Much discussion centered on the problem of the Tyrrhenian stage, its subdivisions, and relative dating. This stage is generally admitted to be complex, but has often been defined on grounds (e.g., altitudinal) other than

the criterion in the original definition proposed by Issel, i.e., as a marine horizon characterized by a Senegalian-type fauna, especially *Strombus bubonius*. The horizon with an indifferent fauna that preceded the deposits with *Strombus bubonius* is sometimes referred to as the Tyrrhenian (= Paléotyrrhénien of E. Bonifay and P. Mars); it is clearly of Middle Pleistocene age, and some workers tend to regard it as contemporaneous with (a part of) the Milazzian, which is known to be complex (multi-cyclic).

Beaches and cliffs cut during the main Tyrrhenian stage, and littoral—and sometimes deeper—sediments of this stage, are widely represented in Spain and the Balearic Islands, southern France, the Tyrrhenian and southern Adriatic coasts of Italy, Corsica, Sicily, Lebanon, Israel, lower Egypt, Cyrenaica, Tunisia and Algeria. The problem is often complicated by the absence of *Strombus bubonius* in certain horizons and regions—in part at least as a consequence of ecological factors—where the stratigraphic evidence strongly favors reference to the Tyrrhenian stage. Comparable littoral formations, stratigraphically of the same relative age, are well developed along the Atlantic coast of Morocco; some slight differences in invertebrate faunas hinder direct comparison and correlation with the Tyrrhenian of the Mediterranean (although some horizons may be traced through the Straits of Gibraltar into the Mediterranean basin).

A number of well-known prehistoric occupation sites, generally caves or shelters, containing basal infillings of marine sediments of the Tyrrhenian stage, occur along the Tyrrhenian littoral of Italy (Monte Circeo, Apulia), at Gibraltar, and in the Lebanon. Their industries are broadly referable to the Mousterian complex, with both stratigraphic and regional subdivisions and facies. Radiocarbon determinations from occupation residues, either directly from such sites, or from other continental sites with similar industries related to long successions of prehistoric occupation, already afford limiting ages for certain of the underlying Tyrrhenian accumulations. The older or *Strombus* horizon (Eutyrrhénien of E. Bonifay and P. Mars) is generally accepted as being of the Last Interglacial age; whereas the younger horizon, situated at a lower altitude (Néotyrrhénien of E. Bonifay and P. Mars), is terminal Last Interglacial to earliest Last Glacial. The relation of the Ouljien horizon (M. Gigout) to either of these horizons in the Mediterranean is not yet absolutely certain. The widespread occurrence of these

horizons, often continuous and undeformed, over substantial stretches of the present Mediterranean littoral, and the accompanying sites of human occupation, are facts of singular importance for paleoanthropological as well as Pleistocene geological investigations.

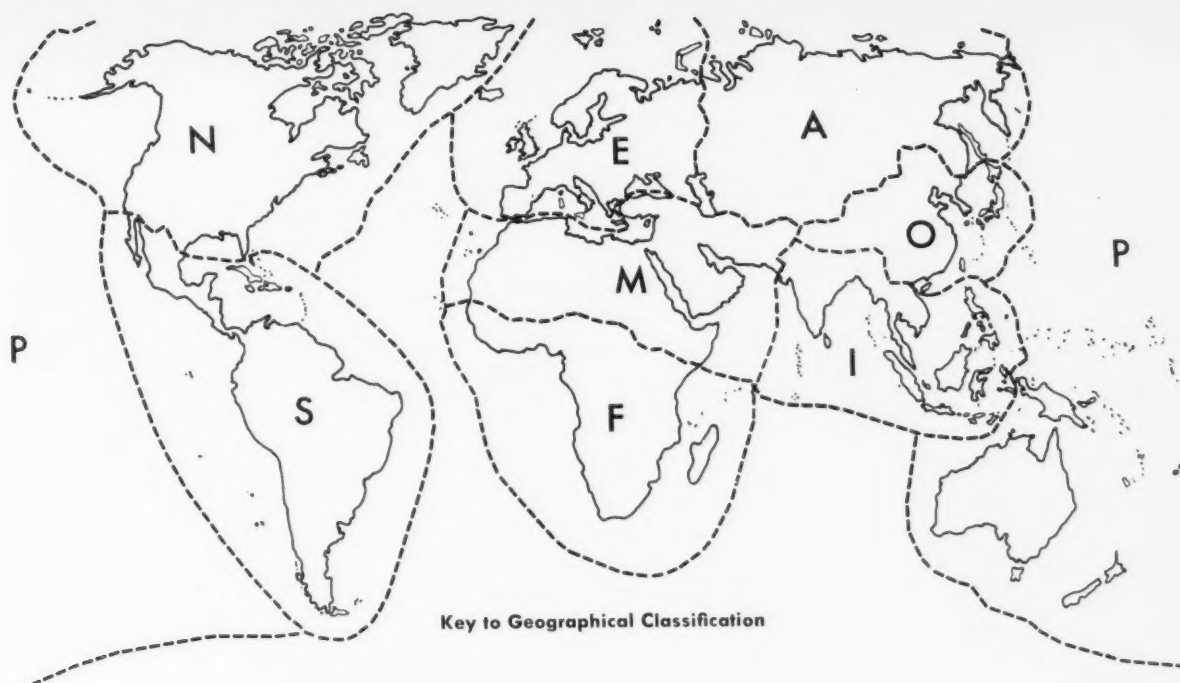
Results:

Among the most important consequences of this conference was the free and continued exchange of empirical data and points of view among workers concerned with research on Pleistocene geology and paleoanthropology in the circum-Mediterranean regions. The nature and direction of future research will be greatly stimulated and guided by these discussions and by the useful background papers prepared by the participants.

Important propositions upon which there was common agreement include the following:

- (1) In the Mediterranean basin and adjacent regions, orogenic deformations, and particularly Epirogenic uplift, occurred throughout the Pleistocene.
- (2) Between the Sicilian and the Tyrrhenian (*Strombus* fauna) marine stages of the Mediterranean occur deposits characterized by an indifferent fauna.
- (3) For reasons of priority (Depéret), this interval can be designated the Milazzian stage although it is not yet known to be represented at Milazzo.
- (4) It is important that further efforts be made to ascertain the stratigraphy of the Milazzian and Tyrrhenian stages at Taranto.
- (5) Near present sea level there are at least two distinct marine horizons of which one represents a late phase of the Flandrian transgression (post-Glacial), and the other is older (terminal Last Interglacial to early Last Glacial). A more intensive program of radiocarbon dating should be undertaken to distinguish certain of these horizons which, in places, may be (and have been) confused because of their similar elevations.
- (6) Workers in the Mediterranean basin and adjacent littoral regions who are especially concerned with the study of the Milazzian and Tyrrhenian stages must have further opportunities to observe such formations in the various regions.

The collected papers of the conference will be published together, probably in a special volume of *Quaternaria*, the journal devoted to Pleistocene geological and paleoanthropological studies, which was founded and edited by the late Professor A. C. Blanc, and will be dedicated to his memory.



Key to Geographical Classification

ASSOCIATES IN CURRENT ANTHROPOLOGY

AS OF JANUARY 1, 1961

ABBREVIATIONS *

aborig.	aboriginal, aborigine
ac.	associate, association
acad.	academy, academia, académie
accult.	acculturation
adm.	administration, administrative
Afr.	Africa, African, Afrique
agr.	agriculture, agricultural
akad.	akademie
Amer.	America, American
anat.	anatomy, anatomical, anatomico
anth.	anthropology, anthropological, anthropologist
antiqu.	antiquities
antiquar.	antiquarian
antr.	antropología, antropologica
apt.	apartment
aptdo.	apartado
arch.	archaeology, archaeological, archaeologist
architec.	architecture
arqueol.	arqueología
as.	assistant, assistante
Aust.	Australia, Australian
ave.	avenue, avenida

B.C.	British Columbia
bibl.	bibliography
biol.	biology, biological
blvd.	boulevard
br.	branch
bur.	bureau
Carib.	Caribbean
cen.	century
ch.	champs
chem.	chemical, chemistry
chrnm.	chairman
chron.	chronology
civ.	civilization
col.	college
colon.	colonial
comm.	community
comp.	comparative, comparison
contemp.	contemporary
coun.	country
cul.	culture, cultural
cur.	curator
dendrochr.	dendrochronology
dept.	department, département
dev.	development, developed
dir.	director
dist.	district
div.	division
e., E.	east, eastern
ecol.	ecology, ecological
econ.	economic, economy, economics, economist

ed.	editor
educ.	education, educational
epidemiol.	epidemiology
esp.	especially
est.	estado
ethnogr.	ethnography, ethnographical
ethnol.	ethnology, ethnological, ethnologie
etnogr.	etnografiske, etnografiska
etnol.	etnologie, etnologia
Eur.	Europe, European
evol.	evolution
fac.	faculty, facultade, faculté, facultà, facultad
fed.	federation
fell.	fellow, fellowship
found.	foundation
gen.	general
geog.	geography, geographical, geographisch
geol.	geology, geological, geologica, geologico
glaciol.	glaciology
gov.	government
grad.	graduate, graduated
hist.	history, historical
hosp.	hospital
i.	island
incl.	including
industrializ.	industrialization
inst.	institute, institution, institut, etc.

* States, provinces, and other political subdivisions are also abbreviated.

instr.	instructor	no.	number	sch.	school
int.	interest	org.	organization	sci.	science, scientific, scientifique, sciencze, etc.
intercult.	intercultural	Pac.	Pacific	sec.	secretary, secrétaire
intn'l.	international	paleoanth.	paleoanthropology	sem.	seminar, seminary, seminario
is.	islands	paleoclimatol.	paleoclimatology	serv.	service, servicio
ist.	istituto	Paleol.	Paleolithic	soc.	society, social
jour.	journal	paleon.	paleontology, paleontologia	socializ.	socialization
lab.	laboratory	paleopedol.	paleopedology	sociol.	sociology, sociologist, sociological
lang.	language	path.	pathology	sr.	senior
lect.	lecture, lecturer	pers.	personality	st.	street
lett.	letter, lettre, letters, lettres	philol.	philology	St.	Saint
libr.	library	philos.	philosophy, philosophical	str.	strasse, straat
ling.	linguistics	phonol.	phonology	stratig.	stratigraphy
lit.	literature	phys.	physical	struct.	structure, structural
math.	mathematics	physiol.	physiology	supt.	superintendent, superintendenza
med.	medicine, medical, medicina	Pleist.	Pleistocene	technol.	technology, technological
Medit.	Mediterranean	pol.	political, politics, politisch, politik	terr.	territory, territoire
mem.	member	pop.	population	transpac.	transpacific
Mesol.	Mesolithic	prehist.	prehistory, prehistorical	treas.	treasurer
met.	metropolitan	prim.	primitive, primitiva	U.	university, universidad, université, etc.
methodol.	methodology, methodological	pres.	president	U.A.R.	United Arab Republic
mid.	middle	prob.	problem, problems	U.K.	United Kingdom
missionry.	missionary	prof.	professor	U.N.	United Nations
morphol.	morphology, morphological	prog.	program	urbaniz.	urbanization
mt., mts.	mountain, mountains	proj.	project	U.S.A.	United States of America
mus.	museum, museo, musée, etc.	prov.	provinces, etc.	U.S.S.R.	Union of Soviet Socialist Republics
museol.	museology, museologist	psychiat.	psychiatry, psychiatrist	v. pres.	vice president
musicol.	musicology, musicologist	psychol.	psychology, psychologist	w., W.	west, western
mythol.	mythology	r.	rue, rua	wiss.	wissenschaftlich
n., N.	north, northern	rd.	road	zoo.	zoological, zoology
nac.	nacional	ref.	reference	&	and, und, et
nat.	natural	relat.	relation		
natl.	national	relig.	religion		
neol.	neolithic	rsc.	research		
		rscr.	researcher		
		s., S.	south, southern		

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- BOWLES, GORDON T. Visiting Prof. Anth., Dept. Anth., Columbia U., New York, N.Y., U.S.A. Comp. ethnogr. & relative significance of soc. insts. & soc. controls in determining man's biol. behavior as an interbreeding animal. Determination of gene pools & the degrees of effectiveness of sex regulations, rules of endogamy & exogamy, degrees of their observance & non-observance & their significance in determining man's heredity. India, Indo-Tibetan & Sino-Tibetan borders, Japan, Korea. **I-O**
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- KEHOE, THOMAS F. Dept. Nat. Resources, Saskatchewan Mus. of Nat. Hist., Wascana Park, Regina, Saskatchewan, Canada. Ethnol., arch., material cul., cul. hist. N. Amer. Plains, N. Woodlands, Arctic, plateau, N. W. Coast., Blackfoot Indians. **N**
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- KELLY, ARTHUR R. Prof. Anth., Phi Kappa Hall, U. Georgia, Athens, Ga., U.S.A. Prehist. Etowah Mounds & relats. to S., Mexicanoid, Huastecan coast. Southeast U.S.A., Mesoamer. influences. **N-S**
- KELSO, ALEC J. As. Prof., Dept. Anth., U. Colorado, Boulder, Colo., U.S.A. Phys. anth. Blood, fossil man, evol. theory. **N**
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- MURDOCK, GEORGE P. Dept. Anth., U. Pittsburgh, Pittsburgh 13, Penna., U.S.A. Prof. Anth. Cul. anth., soc. org. Afr., N. Amer., Micronesia. **F-N-P**
- MURPHY, ROBERT F. As. Prof., Dept. Anth., U. California, Berkeley 4, Cal., U.S.A. Soc. anth. Soc. struct., soc. change. S. & N. Amer. (Plains & Great Basin), Fr. W. Afr. **F-N**
- MURRA, JOHN V. Ac. Prof., Vassar Col., Poughkeepsie, N.Y., U.S.A. Cul. anth. Transition from kinship to state insts., econ. & pol. origin in pre-European times, cul. hist., creativity under conditions of accult. & cul. change. Andes, Carib., W. Afr. **F-S**
- MUSIL, RUDOLF. Dir., Dept. Geol. & Paleon., Moravské Mus., Brno, Czechoslovakia. Pleist. mammalia. Change of animal association in Pleist. Eur., Asia. **E-A**
- MURRILL, RUPERT IVAN. Ac. Prof., Dept. Anth., U. Minnesota, Minneapolis, Minn., U.S.A. Phys. anth., Human racial adaptation, paleoanth. **E**
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- N**
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- OINAS, FELIX J. As. Prof., Dept. Slavic Lang. & Lit., Indiana U., Bloomington, Ind., U.S.A. Ling., folklore. Ling. influence & borrowing. Russian folklore, Balto-Finnic folklore. **E**
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- T**
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- TAYLOR, DEE C. As. Prof. Anth., Dept. Sociol. & Anth., Montana State U., Missoula, Mont., U.S.A. Arch., cul. theory. Ethnogr. Arch. of N. Plains & Great Basin, U.S.A. Ethnol. of Montana Indians. **N**
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Bushmen & Hottentots; fossil man in Afr.,
human genetics, craniology, anthropo-
metrics, Afr., S. Bantu, Bushmen, Hotten-
tots. **F**
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(ethnogr.) sci. Aust., Oceania, North Asia,
Slavic peoples. **A-E-P**
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man sociol. & ecol. Phys. anth., cul. anth.,
comp. sociol. of primates. Amazon, Afr.
S-F
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Ling. Uralic langs. **E**
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Mesoamer., Peru, arch. of U.S.S.R. & N. E.
Asia. **A-S**
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Eur., (Balkans), Eur. U.S.S.R. **E**
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Age. N. E. Iberian peninsula. **E**
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theory, projective testing. Changes in in-
terests over time, large-scale cross-cul.
projective testings.
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relig. Southwest U.S.A. **N**
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seas Chinese soc. org. Village org. Hong
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langs. esp. Latin, Greek, Celtic, Germanic.
Basque, Berber, S. Amer. Indian langs.
W. Eur., Medit., S. Amer. **E-M-S**
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lish, Amer. Indian, lang. & cul. (Kiowa),
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14, N.Y., U.S.A. Ling., cul. theory, Amer.
Indian langs., gen. ling., communication,
methodol. of cul. analysis, ethnol. south-
west U.S.A. The Amers., southwest U.S.A.,
Slavic world. **N-S-E**
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Anth. in educ. W. N. Amer. esp. Pac.
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ment. S. E. China esp. Hong Kong region.
O
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Albanology, Balkans, Asia Minor, Cau-
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S
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material cul. Afr. (interlacustrine re-
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esp. symbolic aspect of cul. Communica-
tion hist. of Modern Japan. **O**
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Natl. des Langs. Orientales. Ethnol.,
ling. Langs. of Ethiopia (Semitic), socs.
that use those langs. (esp. Agaw), Zag-
hawa langs. E. Horn of Afr. (Ethiopia,
Eritrea, Somali countries), Wadday. **M**
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& theory. Field studies of relig. insts. & be-
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ism, ecol. adaptation. Asian arid zone, esp.
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of savage from the civilized, traditionality.
Formosa, Afr. **F-O**
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Mid. East. **E-M-N**
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man problems in technol. change, agr.

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- V**
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Continued from inside front cover

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1961

April 8-9. Catholic Anthropological Conference, St. Louis, Mo., U.S.A. Write: A. Spitzer, St. Louis University, St. Louis 3, Mo., U.S.A.

June 4-10. Third World Congress of Psychiatry. Montreal, Quebec, Canada. Write: General Secretary, Third World Congress of Psychiatry, 1025 Pine Ave. West, Montreal 2, P. Q., Canada.

July 3-9. First Caribbean Conference on Pre-Columbian Archeology. Fort-de-France, Martinique. Write: Historical Society of Martinique, Fort-de-France, Martinique.

July. Inter-African Committee on Linguistics, 2nd Meeting. Brazzaville, Congo. Write: Commission for Technical Cooperation in Africa South of the Sahara, Pvt. Mail Bag 2359, Lagos, Nigeria.

August 21-September 2. Pacific Science Congress. Honolulu, Hawaii, U.S.A. Write: Harold J. Coolidge, Bishop Museum, Honolulu 17, Hawaii, U.S.A.

August 28-September 3. 14th Annual Conference of the International Folk Music Council. Quebec, Canada. Write: Maud Karpeles, Secretary, International Folk Music Council, 35 Princess Court, Queensway, London W.2, England.

September 6-11. 8th Congress, International Musicological Society. New York, U.S.A. Write: Ernst Mohr, Case Postale 154, Basle 1, Switzerland.

September 7-12. 2nd International Conference of Human Genetics. Rome, Italy. Write: Istituto "G. Mendel," 5, Piazza Galeno, Rome, Italy.

September 28-October 1. 5th Conference of Czechoslovak Anthropologists. Milukov near Brno, Czechoslovakia. Write: Milan Dokládál, Anthropological Society, Brno, Komenského nám. 2, Czechoslovakia.

November 16-19. American Anthropological Association. 60th Annual. Philadelphia, Pennsylvania, U.S.A. Write: John Landgraf, Hickory Hill, Tappan, New York, U.S.A.

December 26-31. American Association for the Advancement of Science. Section H (Anthropology) annual meeting. Denver, Colorado, U.S.A.

1962

August 29-September 3. 6th International Congress of Prehistoric and Protohistoric Sciences. Rome, Italy. Write: L. Cardinali, c/o Università degli Studi, Rome, Italy.

September 2-8. 5th World Congress of Sociology. Washington, U.S.A. Write: International Sociological Assn., Skepper House, 13 Endsleigh Street, London W.C. 1, England.

September. 9th International Congress of Linguists. Cambridge, Mass., U.S.A. Write: Miss Chr. Mohrmann, 40 Sint Annastraat, Nijmegen, Netherlands.

November 15-17. American Anthropological Association. 61st Annual. Chicago, Ill., U.S.A.

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